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THE NOVEMBER SCIENTIFIC MONTHLY

EDITED BY J. MCKEEN CATTELL

ON THE NATURE AND THE LIMITATIONS OF COSMICAL INQUIRIES. PROFESSOR P. W. BRIDGMAN 385

CHARACTERISTIC FEATURES OF MATHEMATICS AND OF ITS HISTORY. PROFESSOR G. A. MILLER 398

THE WORK OF THE BUREAU OF STANDARDS IN ELECTRICITY AND RADIO. E. C. CRITTENDEN 405

HOHOKAM: A CHAPTER IN THE HISTORY OF RED-ON-BUFF CULTURE OF ARIZONA. CARL TRISCHKA 417

ANTHONY VAN LEEUWENHOEK AND HIS MICROSCOPES. PROFESSOR G. H. PARKER 434

A CENTURY OF PROGRESS IN THE CHEMISTRY OF NUTRITION. PROFESSOR H. C. SHERMAN 442

SCIENCE AND NUMEROLOGY. PROFESSOR JOSEPH JASTROW 448

SCIENCE SERVICE RADIO TALKS:

CHEMISTRY AND RECENT MEDICAL PROGRESS. PROFESSOR JULIUS STIEGLITZ 451

PESTS AWAY FROM HOME. DR. L. O. KUNKEL 454

THE EARLY IRISH RACE. DR. JAMES A. GEARY 456

NOCTURNAL DREAMS. PROFESSOR WARREN C. MIDDLETON 460

"PEARL" FARMING IN JAPAN. DR. W. O. BLANCHARD 465

PROGRESS OF SCIENCE:

The Science Advisory Board; Newly Elected University Presidents; In Honor of William Beaumont; An Electrical Survey of Islands and Continents in Invisible Films on the Surface of Water; Recent Developments in the Study of Chemical Effects of Alpha Particles and Electrons 470

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THE SCIENTIFIC MONTHLY

NOVEMBER, 1933

ON THE NATURE AND THE LIMITATIONS OF COSMICAL INQUIRIES

By Dr. P. W. BRIDGMAN

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EARLY cosmogonies were almost entirely fanciful in character and were often largely of emotional or religious import. Only within the last few hundred years have we been able to produce cosmogonies with truly scientific pretentions. In the last two decades activity has increased markedly, beginning perhaps with the generalized theory of relativity of Einstein, followed by the work of Eddington, Jeans and Milne on the structure of the stars, and more recently by the theory of the expanding universe of le Maitre and the speculations of Tolman on cosmical thermodynamics. As in physics, probably the chief stimulus to recent activity is to be sought in our rapidly expanding experimental knowledge. It is obvious, however, that there are certain permanent limitations on the possible observational material provided by astronomy; we shall never be able to get into the inside of the stars, for example, or to go back a million years in time. Corresponding to these observational characteristics, there must therefore be certain essential differences between the nature of the structure that we can erect on these observations and other observational disciplines, such as physics, where the observational control is much more far reaching and intimate. In the following I shall attempt to give some analysis of the nature of the structure that can be erected on the observational material of astronomy. I shall seek to find the nature of the structure in terms of "operations"; our study will involve an examination of the nature of the observational material, of the methods used in interpreting this material, and of the methods by which we may check the validity of any solutions that we may find.

The observational material is subject to one obvious and essential limitation in that it is almost entirely optical in character. The only exception consists in the examination of actual celestial material which reaches the surface of the earth in the form of meteorites. Knowledge of this sort is at present very scanty, although it is daily becoming more important; it will always, however, be subject to obvious vital limitations due to the small size of any such meteoric material with which we can deal. The optical material consists, in the first place, of observations of the direction of the heavenly bodies; secondly, of the intensity of the total radiation from each individual object; thirdly, of the spectral characteristics of this radiation; and finally, in a few cases, of interference or other phenomena involving differences in the character of the radiation from different parts of the same object.

Measurements of direction are of high

precision, down to a small fraction of a second of arc. This is sufficient, by the method of parallax, to give fairly good information about the distances of the nearer objects. No measurement of astronomical distance, however, has an accuracy of much more than 0.1 per cent. Beyond the range of parallax, observation gives merely a projection in two dimensions of position, and distance has to be estimated by indirect methods involving extrapolations of observations made in the parallax zone, where it is possible to test the legitimacy of the method. Perhaps the most important of these methods is the estimate of the distance of the Cepheid variables in terms of an empirical connection between the size and the period established in the zone of parallax. This method is used by very wide extrapolation. Any such extrapolation must be prepared to defend itself against the criticism that there may be systematic changes at great distances in the connection between period and size. Such a defense is impossible until we have more exact knowledge than at present of the mechanism responsible for the variation of the Cepheids, and in any event a defense would always be impossible against the suggestion of the existence of effects at present unknown. We have to make our extrapolation as we do because in our ignorance we are powerless to suggest any plausible way in which the observed connection between size and period might be different.

The astronomer is forced to go even further than this, however. Much observation has developed in him certain "hunches." For instance, if he sees projected on the celestial sphere a large number of stars, far beyond the parallax distance, all grouped within a small disk, such as would be the case if he were observing a globular cluster, he will reverse the argument and say that the observed stars are actually grouped together in space into a globular cluster. Further, if he is fortunate enough to find within the group Cepheid variables, he will fix the distance of the whole group by the extrapolated distance of these variables. If the distances of all the Cepheids turn out to be nearly the same, the assumption of an actual globular cluster is felt to have a high degree of "probability."

By hook or by crook, therefore, the astronomer is able to assign distances to a large part of his observed objects; the confidence which he feels in these distances decreases as the absolute value of the distance increases.

The distribution of light in the spectrum of each star gives information about the mean temperature of the effective radiating surface and the chemical composition of this surface. Inference as to these quantities is made in large part on the basis of laboratory measurements, but also, especially as to the effective temperatures, on the basis of theory by which the effects of temperature and pressure on the dissociation of the elements may be extrapolated to temperatures and pressures beyond experimental reach. The theory involves quantum mechanics and thermodynamics, and we are fairly well satisfied with it, for the extrapolations are not particularly large as such things go. The inferences about the chemical composition are also satisfactory; in fact I think any astronomer or physicist would regard this as perhaps the most satisfactory part of our information. But even here, the assumption has to be made that the laws which we find in our laboratory experiments are not essentially modified under the extremely different stellar conditions, and I suppose it would be impossible to give a thoroughly logical answer to a carping critic who wanted to insist that this is really only an assumption and might not be legitimate. The carping critic will, however, find it impossible to convince a physicist that 5,000 spectrum

lines, all in approximately the same position, do not mean the same element, iron, in the star that they do on the earth. The argument again here rests on considerations of probability, which appeal to every one with scientific experience, but which nevertheless involve vague and imperfectly understood ideas which have so far eluded precise formulation.

The optical data sometimes permit estimates of the angular diameter of the star by interference methods, but the estimate of size is usually more indirect, involving the assumption of the universality of various empirical relations which are capable of direct verification in only a few favorable cases. At any rate, whatever the details of the method, the astronomer has estimates of the size of a large number of the stars, of precision much less than the precision of the best distances.

The mass of a star may be determined if we are fortunate enough to be dealing with a star which constitutes one member of a binary, the period and the separation of which may be observed. The mass of a large number of stars has been determined in this way. In calculating the mass from these data, the inverse square law of gravitation has to be assumed, with the same constants as observed in the solar system, and again there is no defense against the criticism of possible changes in the law of gravitation at great distances or in remote epochs of time.

Finally, observations of position at different times are accurate enough to give the apparent motions of a number of celestial objects; these combined with distance give the velocity in space perpendicular to the line of sight. Spectroscopic observations of the Doppler effect give the radial components of velocity, assuming that we know how to separate the Doppler effect from other effects giving rise to spectral shifts. Altogether, there is a rather large amount of material permitting us to give motions in space of varying degrees of accuracy.

Besides the characteristics of the data of astronomy already discussed, there is one other very essential characteristic, in that it is all "observational" in the narrower sense. We have to take what we find, without the possibility of studying what happens when we vary the conditions, which constitute such a powerful method in physics and chemistry. Astronomy is the observational science *par excellence;* geology is much like it but with growing possibilities of experimental control afforded by geophysics, and perhaps we may next place biology, until recently an observational science, but becoming rapidly completely experimental.

Such is the nature of the observational material and the inferences it allows as to distances, sizes, motions, compositions and physical state. Our problem is now to organize this material and bring it into coordination with other branches of knowledge in such a way that we feel that we understand the resulting structure. This we may perhaps define as the problem of cosmogony in the broadest sense. In the working out of the problem we may perhaps recognize two main aspects. There is in the first place the problem of inferring from our observational material what the present complete structure of the universe is; this involves, for example, determining the nature of the inside of a star, and in general inferring the unobservable parts from what we observe. Then in the second place there is the problem of extrapolating our observations backward and forward in time in order that we may get some idea of where our universe came from and what is its future. This latter is by many regarded as the most interesting task of cosmogony. This extrapolation forward and backward should, if the present configuration is completely known, be capable of unique

performance, so that we should be able to obtain a unique answer to our questions about past and future. But in view of the extreme incompleteness of our understanding of the present configuration, this ideal is very far from attainment. In fact, instead of making a straightforward extrapolation from the present configuration, the process is often reversed and inferential information about the present configuration obtained on the basis of what would be involved in the way of extrapolation. There are, in the first place, the data of geology controlling any extrapolation; it is obvious that our backward extrapolations must provide a long enough past for the demands of the geologist, and any limitations which requirements of this nature impose may be accepted as perfectly legitimate. We may be grudging in our acceptance of this control, however, because we recognize that we ought to be able to reproduce the past of geology without aid from this adventitious course. Aside from this perfectly legitimate demand which we make on our extrapolation, we make other demands which we would find it harder to justify on any scientific ground. For example, we probably shall eventually reject any reconstruction of the past as highly "improbable" which represents all the matter of the universe as having originally constituted a single giant atom, in spite of le Maitre, and I am perfectly sure that any extrapolation into the future which predicted the destruction of the entire cosmos within 1,000 years would be at once rejected, although perhaps unconsciously, as being derogatory to human dignity.

In general the problem of cosmogony is so difficult that no sort of control or argument appears to be neglected that can have any bearing on the problem, even if the argument is as purely emotional as that just suggested.

We are now ready to consider the details of the attack on the problem of cosmogony, and it is natural to first set ourselves the problem of understanding how the individual units, the stars, are constructed. What we have given is the surface temperature, chemical composition, total size and total mass, and we are required to reconstruct the state of affairs at every interior point. At any interior point we must know the temperature, composition and pressure, and we must know how these quantities vary from point to point. The point to point variation is controlled by the differential equations of mechanics and thermodynamics, into which enter the parameters which determine the pertinent physical properties of the stellar matter, such as emissivity, absorption, etc. We assume that our knowledge of the nature of the material is sufficient to determine these parameters when temperature, composition and pressure are given, but it is only recently that enough data have been accumulated in the physical laboratory to give us the slightest confidence in our assumptions about these quantities. The problem in general features is not unlike other problems to which physics can give an exact solution, such, for example, as the problem of elasticity. In elasticity theory we are given a body of known physical properties subject to a given set of forces acting over its external surface, and perhaps other forces like those of gravitation acting on the material inside, and we are required to find the stress and strain at every interior point. It is possible to give a mathematical proof that a unique solution exists. Our physical intuition leads us to expect similarly a unique solution for a star, but it is still far from evident what the exact mathematical conditions are which would lead to such a unique solution. In fact the general problem of finding the appropriate mathematical method by which to attack this problem would seem not yet to be definitely settled.

Certain plausible mathematical formulations have been rejected simply on the basis that in the solution they lead to infinities at the center. Now an infinity in a solution is no reason for rejecting an equation which leads to it. The validity of the equation is a matter which permits of direct test, and if under some conditions infinite solutions are demanded, the conclusion is that the physical assumptions which were at the basis of the equations have ceased to be valid, and must be replaced by others, the original equation still retaining its validity over the range within which it was established. The procedure by which the straightforward solution of the equation which naturally presents itself has been replaced is a complicated one of trial and error, starting with assumed conditions at the center and working out until we find conditions at the exterior as nearly as possible like those observed. Success has not been impressive and it is not certain that a single type of equation is adequate for the solution.

The mathematical difficulties in the way of the construction of a model of a star are therefore severe enough; the physical difficulties are not less formidable. The mathematical solutions, which have been obtained thus far, agree in assigning to the interior of the star perfectly scandalous temperatures and pressures—temperatures of the order of tens of millions of degrees, and pressures of the order of tens of millions of atmospheres. Such conditions are so tremendously beyond the range of anything that can be obtained in the laboratory that any extrapolation of properties observed in the laboratory must be viewed with extreme suspicion, even when this extrapolation is made with the help of the best theories that we have at present. Entirely unknown sorts of behavior of matter under these extreme conditions are to be expected. The recently discovered dense stars show definitely enough the possibility of matter existing in hitherto unknown dense forms, and it is not yet agreed whether we are to expect a core of such dense matter at the center of each star or not. The recent discoveries of the neutron and positive electron also tremendously affect the possibilities and there is the possibility of elements of very high atomic weights, not hitherto known. Ignoring these difficulties, however, models of a sort have been constructed which have been successful in reproducing some of the outstanding observational regularities of the stars, such as the relation discovered by Eddington between mass and total brightness.

In spite of all the vagueness of our present formulations and our ignorance of the physical conditions inside the star, which we might be inclined to think would leave open sufficient possibilities to permit an eventual exact solution, every stellar model at once encounters one difficulty which peremptorily forces us to abandon our original program of completely reconstructing the star in terms of data of the laboratory. The difficulty, of course, is concerned with the radiation. The rate at which a star is losing energy by radiation is capable of measurement. (This involves the assumption that the radiation in every direction is the same as in the direction of the earth. Attempts have been made to postulate that radiation takes place only in the direction of bodies in position to receive the radiation, but this possible way out has encountered insuperable difficulties, such as that of accounting for the radiational equilibrium of the earth.) The total amount of energy radiated in geological time can therefore be calculated, and this turns out to be much greater than accounted for by any known source of radiational energy. It is not necessary here to assume a complete law of conservation of energy—the energy concept

encounters logical and formal difficulties when we attempt to apply it to systems in which only uni-directional processes are taking place. All that is necessary is the observational fact that there is no known source of radiation which does not involve the exhaustion of its source; energy is radiated only at the expense of some sort of permanent change in its source. It is perhaps worth mentioning in passing that the times involved are so long that the direct observation of the cooling or other exhaustion of any celestial body in consequence of its radiation is hopelessly beyond us. Granting, however, that radiation means exhaustion celestially as well as terrestrially, an exploration of all known sources of energy discloses that they are inadequate. It seems unavoidable therefore to assume some unknown source. It happens that our present experience contains the suggestion of one possibility which, although not actually realized, does not demand a very revolting extrapolation of laboratory conditions. This is the transformation of matter into energy, first suggested by Einstein's relativity theory, in which it appeared that radiation of energy was accompanied by loss of mass of amount $m = E/c^2$. This equivalence of mass and energy has been established with high probability for the radioactive transformations. The largest effect of this kind to be expected cosmically is the change of mass accompanying the transformation of four hydrogens into one helium. This transformation perhaps provides an adequate source of energy, but the margin of safety is not large, and a number of cosmologists do not feel that this is the solution. They prefer to go further and find the source of energy in the complete annihilation of matter when electron combines with proton. This very probably provides an adequate source. But on the other hand, the process has never been observed and is purely inferential; the great argument is that it fits neatly into our mathematical formulation of relativity theory.

In any event the important feature for us is that we have unequivocally encountered a situation which shows that our original program is not capable of execution, but we must assume processes not directly given to us by laboratory experience. Having thus once let down the bars, a flood of possibilities descends upon us, and the question is, where are we going to stop? There are many other places where it would be plausible to look for unknown processes. What is the basis for assuming that the conservation of energy is sacrosanct? Why may we not simply postulate that energy is created in a star? Inside a star radiation is continually being emitted, traveling a short distance and then being reabsorbed. Suppose that conservation fails by a small percentage amount at each process. One can get some idea of the relative number of total emission processes going on in the star compared with the number which are concerned in the net radiated energy by considering that in the sun the ordinary thermal content would supply the radiational energy for 15,000,000 years, and that a beam of light travels the solar radius in something of the order of 2 seconds. If conservation at each elementary quantum act of emission and reabsorption fails by an amount fantastically beyond the possibility of direct experimental verification, the total net radiation is adequately accounted for. Or again, how accurately is the inverse square law of gravitation checked by any direct observation under conditions approximating those in the interior of a star? What basis have we for thinking that gravitational screening may not be a property of matter with density 50,000 times that of water, when claims have been made that the effect has been detected under terrestrial conditions? What is the basis for thinking that the

ordinary laws of electrodynamics retain their simple linear form when electron and proton are as close together as they must be or when we have as intense radiational fields as in the stars. Physicists are already openly discussing the necessity of modifying the accepted quantum laws for the nucleus. Why may not matter even be created inside the stars? The universe had to come from somewhere.

When one considers the criteria by which he attempts to judge the seriousness of these various possibilities I believe that it must be conceded that the whole matter is rather vague. Probably the broadest characterization of the criterion which we more or less unconsciously apply is that of simplicity, the same criterion which we use in other speculative work when the problem is undetermined. But simplicity has no absolute meaning; what may be simple from the point of view of a mathematical equation may be more complicated when the procedure is formulated in words or translated into physical operations. An example is the procedure for measuring the length of a moving object in relativity theory; the actual definition corresponds to simple equations, but is much more complicated when expressed in words than the naïve and rejected procedure of measuring the moving object by the application of meter sticks moving with the same velocity. By what right does the mathematical formulation acquire precedence? In this example the situation is completely determined and a decision may be made, but no such decision is possible in cosmogony.

In describing ordinary physical experience rough criteria of simplicity have stood the check of workability because it has been found possible to reproduce our physical experience in simple terms. Without such an experimental proof of the possibility of simplicity it would seem that there can be no logical reason for expecting simplicity in preference to complication. Now in the cosmic case the check against experience, in order to choose between rival theories, is not possible, each theory being constructed so as to be equally capable of dealing with the facts, and the criterion of simplicity loses objective probability. But other physical experience would suggest that in this realm of totally unknown conditions, pressures of 10^7 atmospheres and densities of 50,000, nature would be expected to become more complicated, and simplicity would be expected to fail. We might argue, therefore, that the more complicated of two rival theories would have the greater chance of being correct. But in the absence of specific indications of the direction which the complication should take, we must show no preference for one complicating hypothesis as distinguished from another, so that an attitude of perfect neutrality demands that we reject every complicating hypothesis that we can and thus revert to simplicity. In this domain, therefore, our expectation of simplicity in the physical phenomena fails us, whereas simplicity in our theories remains because it expresses most perfectly our complete ignorance.

It may happen that different models are equally capable of reproducing the present observational properties of the stars, but when extrapolated in time different pasts or futures may be demanded. Granted that the single condition is satisfied that a star like the sun has a tolerably constant past as far back as geological time, what further criterion in addition to those already discussed shall be applied in making our selection? One criterion of intrinsic probability is very definitely applied in practise, namely if any solution shows scattered throughout the universe stars in different stages of evolution, satisfaction is felt. Thus if my solution for a

present red giant indicates a condition at some future time such as I now find in some white star, I am pleased, and feel that my solution has a better chance of being correct. That is, it seems probable to me that the stellar universe as at present observed contains stars in different stages of evolution. This involves the thesis that the stars did not all come into being at the same time, but successively. Why does this seem more plausible than the assumption that they all started together? I think examination will show that there is nothing very rigorous here, and that the argument is to some extent a "hunch" argument, resting partly on the feeling that in the nebulae we are observing localities in which stars are actually being born at the present time.

The problem of the structure of a star is the problem of the atom of astronomy. Besides this there is the problem of the aggregate of the stars, just as we have a kinetic theory of gases in addition to the problem of atomic structure. In kinetic theory the precise structure of the atom makes little difference as long as the atom has certain very general properties of elasticity, etc. In the cosmic case there is a closer connection between the problem of the structure of the unit and of the ensemble, for the life of the ensemble extrapolated backward must not be shorter than the life of the individual units. There is obviously no such corresponding condition in kinetic theory. For the present, however, we neglect this aspect of the problem, and confine our attention to the problem of finding the positions of the stars in past and future time, treating the stars as unalterable units.

The first impression on approaching this problem is, of course, one of overwhelming complexity; mathematics has not been able to completely solve the problem of even three bodies moving under their mutual gravitation. Some sort of simplification is obviously necessary and we must be satisfied with only a rough answer to our problem. The first question is: what data are necessary in order to ensure a determinate answer? If the problem were one of ordinary mechanics, in which the mutual forces between the stars were known as soon as their mutual distances were known, it would be sufficient to give the present positions and velocities of all the stars. This we may hope to approximate observationally, although the velocities are determinable only very roughly. But the stellar problem is not a problem of ordinary mechanics, because the distances of separation of the stars are so great that the time of propagation of gravitation is important. The analogue of this problem is the retarded potential problem of electrodynamics. Here the solution is not determinate unless the initial positions and velocities of all the particles is given, and in addition the initial values of the electric and magnetic fields at every point of space. The analogue for the cosmic problem of this last requirement is a knowledge of the present gravitational field at every point of space. But the gravitational field throughout space is not observable, so that in principle the problem of extrapolating backward or forward in time is indeterminate, and the most that we can hope for, even with infinite mathematical skill, would be an approximate solution in which there is an indetermination which would be expected to grow in importance as the interval of time becomes longer. I do not know whether a quantitative discussion has been made of the importance of this neglected factor or not.

There are other mathematical difficulties in the case of even so comparatively simple a system as the solar system. Brown is authority for the statement that we can not be sure of gravitational stability for more than 10^8 years. This

is much less than geological time, so that apparently the mathematical extrapolation runs into difficulties in times that are shorter than we know are necessary. One can never hope, therefore, to get a rigorous solution back to the time required. Another difficulty has been pointed out by Brown, who thinks that the equations of gravitational motion of so complicated a system may have such a high number of singularities that it is observationally impossible to supply the number of positions and velocities necessary to determine a solution.

There is another difficulty of a fundamental sort. The Heisenberg principle of indetermination does not permit of the simultaneous observations of position and velocity which are necessary for extrapolation, but there is a necessary reciprocal error in the observation of these two quantities, which means an error growing with time in any possible extrapolation. Over longer and longer intervals of time the details become more blurred, until the actual identity of the objects that we are talking about is lost, and we could not tell, for example, whether a star which we extrapolated to be in a certain position in 10^{50} years was Capella or Vega. Prediction does no good if we have no way of telling what object it is that we are talking about, so that we may say that it is meaningless to attempt to extrapolate time so far forward or backward. Or in other words, the concept of time itself fails if the extrapolation is too extensive. This of course all assumes the validity of the Heisenberg principle when applied to the motion of the stars. It is perhaps needless to say that there is not a scrap of experimental evidence for this, so that our application of the principle is nothing but an enormous extrapolation of a mathematically simple law, made in the same spirit as our assumption of a mathematically exact inverse square law or exact conservation of energy for individual quantum processes.

In spite of all these unavoidable vaguenesses in the extrapolations of positional astronomy, there are certain facts of observation which can not hide behind the possibilities left open in this way, but they indicate the need of some drastic rearrangements in our ordinary explanations, just as the radiational difficulty with the single star demanded the discovery of some new source of energy. The most striking of such phenomena is perhaps the shift in the relative angular position of the stars when their light passes close to the edge of the sun. The only solution proposed which has been at all well received is that of Einstein in his generalized theory of relativity. This has demanded a radical reconstruction of the four dimensional union of space and time with which special relativity theory was satisfied, and involves the idea of a curvature of space-time in the gravitational field. There is confirmatory evidence of this solution in the shift in the perihelion of Mercury and the displacement toward the red of lines radiated in an intense gravitational field, but these checks are not so striking as the other. In any event the entire scheme of explanation suffers from the logical difficulty that it is impossible to prove a curvature of space-time, which requires an infinite number of parameters for adequate specification, in terms of only three observational quantities. The fundamental assumption that space-time is curved can not be directly tested; for one thing, the space of astronomy is only optical, and the equivalence of optical and tactual space can never be adequately checked. The uniqueness of Einstein's solution can never be established; the argument for it, as in so many other cases, can only be the argument from simplicity in the mathematical formulation.

The differential equations of general relativity theory are not sufficient to completely solve the problem of the entire cosmos, but integration constants

appear for which we can offer no physical explanation, but which have to be accepted merely as brute facts. It is well known that entirely different behaviors are indicated in the stellar system at great distances accordingly as a positive or negative value is assumed for one of these constants, and that the observational material is not adequate to allow a choice. The expanding universe of le Maitre results from a particular choice of a constant of integration at a particular stage of the solution. It has been interesting in the last few years to follow the way in which opinions about the probable values of the constants have followed our increasing observational knowledge. We now have the experimental fact that the radiation emitted from the most distant objects is shifted toward the red by amounts indicating enormous velocities of recession if the ordinary Doppler explanation of the shift is accepted. But this property of radiation from distant objects can also be explained in terms of a proper space-time curvature, with the proper constant of integration. On what basis shall we choose one explanation in preference to the other? The preference at one time seemed to be in favor of a curvature, but at present it seems to be for a recession. The reversal of preference has been brought about by the discovery by le Maitre of a method of getting an expanding universe into a comparatively simple mathematical scheme. I believe that if a mathematician or astronomer were made to defend his present preference he could not do better than to urge that it seems *more probable* that the physical state of affairs should correspond to a mathematically simple scheme than to something more complex.

The same argument from feelings of probability occurs in many other places in astronomy. It is justifiable when one is engaged in statistical studies, comparing one collection of objects from one part of the sky with another from another. But in strict logic probability considerations apply only to large numbers or when it is possible to repeat the experimental conditions a great many times. The idea of probability never applies to individual physical situations; probability considerations can not apply to a single throw of dice, for example. The stellar universe is all there is, and it makes its history only once. Probability considerations do not properly apply to it at all. It would be most difficult to analyze just what is involved logically in the frequent application of this sort of an argument to the cosmos. I suspect that at bottom the argument would be found to revert to the old argument from simplicity, and to those hazy arguments, popularly phrased in the language of probability theory but logically quite different, by which we determine our course of conduct when we are confronted with situations about which we are largely ignorant.

We have thus been driven in our positional astronomy to postulate new effects, not directly verifiable by observation. But are there not other unobserved and new effects which we would expect on general grounds? Must we not assume that there are places where electrons and protons come into being? What about radiation? Does it come back? Does it recombine to give proton and electron? What is the significance of the cosmic rays? May not all our physical laws be undergoing a course of evolution, with slowly changing values of all constants?

The difficulties which we have thus far discussed are to a greater or less extent concerned with the details of working out various specific aspects of our problem, but there are more fundamental difficulties, already hinted at. One may well question whether the very concepts in terms of which we do our thinking are adequate to the situation. We have al-

ready met an example that suggests the sort of failure that we may find in our concepts. The energy concept of thermodynamics is rigorously defined in terms of processes which involve bringing the system back to its initial configuration; this is obviously impossible when the system is the entire cosmos, so that in the strict sense of thermodynamics one can not talk about a conservation of the "energy" of the universe. In this particular case we have seen that the formal failure of the energy concept is not of much importance, because for our purposes we can get along with only a partial aspect of the energy concept, and it is possible to find an ideal meaning for this partial concept in its application to the entire universe. But it is not so evident that we would be similarly successful in side-stepping the difficulties of other concepts. Practically every one of our physical concepts demands the performance of an experiment, which in the first place can be indefinitely repeated in time, and which in the second place involves dividing the universe into two parts, one isolated from the rest, on which experiments are made by an external agency, whose actions are supposed arbitrary and unaffected by what occurs inside the isolated region. This procedure evidently breaks down when the subject is the entire universe. How, for example, shall we define the mass of the entire universe to the satisfaction of a critic who insists that the mass of the whole is not the sum of the masses of the parts, a fact which we ourselves very well recognize even in small parts of the universe when we make our measurements accurate enough.

There are particular difficulties with the concept of time in addition to the possible difficulties connected with the Heisenberg principle, already mentioned. The crude difficulties of the naïve time concept have been long felt. Such a difficulty has already appeared in a loose phrase purposely introduced earlier in this paper when it was asked "May not matter even be created inside the stars? The universe had to come from somewhere." We are here face to face with the age-old dilemma; we demand an antecedent for everything of our experience and again an antecedent for the antecedent and so on in never ending regression, and on the other hand we demand equally insistently something in the beginning to start things off. The only answer is the brutal and unsatisfactory one that the urge of our minds to act in this way must be resisted because it does not work; the origin of the urge is in limited experience, and we must recognize that such experience may land us in inconsistencies when pushed too far. The only precise way of dealing with the concept of time is the operational one to which all modern physics seems to be driving us, and which I have expounded in other places. What do I mean from this point of view by the future? In the strictest sense what I at the present, as I sit in this chair, mean by the future is nothing but the complex of expectations which I can formulate to myself at the present, at which I arrive on the basis of all the regularities in nature which my past experience has disclosed to me. The operational meaning of the future, for me in the present, is the complex of those operations by which I deduce my expectations. But this severe meaning of the future will, I believe, be found to not quite correspond to what most people want to mean. We can go part way toward meeting this instinctive feeling by admitting another operation to our armory, the operation of waiting. The operational meaning of saying that I shall have lunch at 1 o'clock is that if I perform the operation of waiting until 1 o'clock I shall have certain experiences. There are many difficulties with this operation of "waiting" and I feel

that it is dangerous to admit it, but for the present purposes we may perhaps allow ourselves this liberality. The important immediate point for us is that none of us, I as I write, or you as you read, can possibly wait beyond the termination of our individual lives. Any possible meaning that the future can have for any individual must be sought in the things that can happen to him in his lifetime, or in an interval of not more than 200 years, a rather brief interval compared with astronomical times. Contemplation of this broadest possible meaning that we can give to the future will give, I believe, a rather different feeling about the significance of the predictions of astronomy. In physics the situation is qualitatively entirely different, for all the operations by which the fundamental concepts are defined and which are involved in the fundamental experiments are of such short duration that the "waiting" aspect hardly presents itself. However, the waiting aspect is always present to some extent, because operations are performed in time, and are composed of parts described in a prescribed order, so that on beginning a complicated operation one has to wait before one can perform the last part of it. Again, as so often before, we see that there are no hard and fast lines of demarcation between regions of validity of concepts.

With regard to past time, the operational meaning of the past is much more definite than that of the future, for there is no possibility of a questionable operation for the past corresponding to "waiting" for the future; I can never penetrate back into the past. The past for me, as I sit in my chair, means simply the aggregate of those reconstructions which I make on the basis of all the experience now at my command. It is evidently a most complicated thing, but is always subject to the limitations to which the reconstructions which I can make are subject. It means nothing to ask what was the past "actually," unaffected by the limitations of thought in reconstructing it. A proper realization of this again gives one a rather different feeling as to the significance of the extrapolations of cosmogony into the past.

Finally, I attempt to summarize the view to which this analysis leads us as to the nature of cosmogony. The essential limitations of the experimental material place cosmogony in a class by itself. It partakes as much as possible of the nature of the completely experimental subjects, physics and chemistry, but is compelled by necessity to introduce features relating to less sharply defined human activities verging into the artistic, the emotional and the metaphysical. The artistic instinct in the cosmologist finds expression in selecting those formulations or solutions, out of the many possible ones, which are most elegant or most simple. The emotional element is well illustrated by the attitude which various cosmologists take toward extrapolation into the indefinite past or future. There are diametrically opposite attitudes here. Thus to Tolman it is extremely repugnant to think of the universe being carried in time to such a configuration that extrapolation can be carried no further, which means either an act of creation at this epoch, or at least a fundamental change in the laws of nature. To Tolman the idea of a special situation presented by the problem of evolution which demands the assumption of new and unknown laws is so abhorrent as to make the whole picture almost unthinkable. This has driven him to hunt for oscillating solutions of the cosmic problem, which permit the universe to oscillate back and forth between extreme configurations, indefinitely repeated both in past and future, so that there is no limit in time either forward or backward. On the

other hand, to a man like Eddington, the idea of a universe continually oscillating between extremes is irreconcilable with feelings of a purpose in stellar evolution. Eddington exclaims that he is an "evolutionist, not a multiplicationist." He prefers a solution in which a definite act of creation has to be assumed in the past, or at least some sort of catastrophe initiating the present régime, and perhaps a gradual decline to the "heat death" of Boltzmann.

The metaphysical element I feel to be active in the attitude of many cosmologists to mathematics. By metaphysical I mean the assumption of the "existence" of validities for which there can be no operational control, a statement which in itself is almost meaningless from the operational point of view, and in fact I personally have no feeling for this sort of thing, and can only use the term "metaphysical" in describing behavior which I observe in other human animals. At any rate, I should call metaphysical the conviction that the universe is run on exact mathematical principles, and its corollary that it is possible for human beings by a fortunate *tour de force* to formulate these principles. I believe that this attitude is back of the sentiment of many cosmologists toward Einstein's differential equations of generalized relativity theory—when, for example, I ask an eminent cosmologist in conversation why he does not give up the Einstein equations if they make him so much trouble, and he replies that such a thing is unthinkable, that these are the only things that we are really sure of.

I believe that there are dangers in any subject in which there is such an unavoidable mixture of purely "scientific" and "human" elements. It seems to me that there is particular danger of introducing actual inconsistencies into the structure if the metaphysical attitude with regard to mathematics is so far adopted as to obscure the perfectly legitimate use of mathematics in attaining simplicity of formulation. The dangers, I hope, may be minimized by discussions like this. But even a perfectly clear-eyed consciousness of the nature of cosmogony will still leave it a subject in which are inextricably mingled together the austere aspects of a purely scientific subject with the warmer and incalculable aspects of a "humanity." To many persons this will constitute an abiding source of fascination; it has at least added to the pleasure of this attempt at analysis.[1]

[1] After sending the manuscript of this article to press, I have read the inaugural lecture of Professor H. H. Plaskett, at Oxford University on April 28, 1933, entitled "The Place of Observation in Astronomy." There are many similarities between his points of view and those above.

CHARACTERISTIC FEATURES OF MATHEMATICS AND OF ITS HISTORY

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WHAT machines are to the industrial world mathematics is to the thought world. Just as a machine is useful only in the hands of an intelligent guide, so theoretical mathematics is really useful only when its postulates are understood and its conclusions are construed in harmony therewith. Both are employed largely because they economize effort. They are, however, used also because they enable the human race to accomplish what would otherwise be impossible. For instance, the rapid transportations of to-day would be impossible without machinery, and the measurement of distances to and between heavenly bodies would be impossible without mathematics. The modern industrial world finds machines indispensable and the modern intellectual world has enjoyed the fruits of mathematics too long and in too many ways to entertain seriously the idea of returning to a thought world devoid of mathematics, even if individuals frequently do this after their college days.

According to Pappus, the simple machine known as the lever impressed Archimedes so forcibly that he said, "Give me a place to stand on and I can move the earth." Similarly, the Pythagorean philosopher Philolaus is said to have remarked that "All things which can be known have number; for it is not possible that without number anything can either be conceived or known." The fundamental place of the lever in machinery corresponds to the fundamental position of the concept of number among the mathematical concepts, and the recognition of the importance of these concepts may have given rise to the outbursts of enthusiasm just noted. Primitive people sometimes combine even yet the name of a number so closely with the counted objects that they use different words for such numbers as 4 and 10 when they are applied to different objects.[1]

Such a waste of effort could not be tolerated indefinitely by the advancing sciences and the use of the same name for the number 4, for instance, independently not only of the objects counted but also of any concrete objects, exhibits a fundamental feature of the mathematical machines. The operation with abstract numbers is a man-made device to deal at a stroke with operations relating to an endless number of different objects. It would be difficult to find in the industrial world a more striking example of a machine which enables the individual to work with speed and great power. Abstraction in the intellectual world corresponds to power in the physical world, and mathematics is preeminently the science of the abstract which was inaugurated by the concept of abstract number in prehistoric times and has been greatly enhanced since then by the broadening of this concept and the introduction of others.

The use of unit fractions is one of the most striking examples in the history of mathematics of the suggestion of an elementary theorem for thousands of years before it was actually proved. In all the

[1] *Cf.* E. Fettweis, "Das Rechnen der Naturvölker," 1927, page 3.

cases which appear in the table at the opening of an ancient Egyptian work, all the denominators in the same problem differ from each other. It was, however, not explicitly proved in the literature before 1931 that a necessary and sufficient condition that a number is rational is that it is the sum of a finite number of distinct unit fractions and that these addenda can always be selected in an infinite number of different ways so as to be equal to any given rational number.[2] One of the characteristic features of the history of mathematics should be an emphasis on the suggested theorems and the misconceptions. From this point of view the theorem just noted is one of the most important relating to pre-Grecian mathematics and the view still held by L. Euler that every negative number is greater than infinity is one of the most important features of the history of negative numbers. Such features make the history of mathematics a study of thought development by the human race.

Calculating machines relate to only a small part of mathematics and effect a comparatively small saving of labor. The great saving is of a more theoretical character and underlies the creation of the abstract numbers which are utilized by the calculating machines. Hence our topic has only slight contact with these devices. While these machines began to be developed in the seventeenth century the abstractions upon which their usefulness largely depends were practically completed when our earliest extant mathematical records were made. The labor-saving feature of mathematics is more forcibly exhibited by the theory of permutation groups to which have been reduced not only various important questions relating to the theory of equations but also such a somewhat trivial question as the possible arrangements of the players at card tournaments when the number of players is of the form 2^m so that during $2^m - 1$ games each of the players shall have each of the others once as a partner and twice as an opponent.[3]

The history of group theory illustrates very clearly the fundamental principle of the history of mathematics that many simple theorems failed to command respectful attention until it became known that they are special cases of more general useful ones. These more general theorems were explicitly noted and led to explicit extensions not only upwards into their more complex ramifications but also downwards into their obvious details. Just as some people attain respectability only on account of their prominent close relatives, so some mathematical ideas have obtained positions among the more important results solely on account of their close connections. For instance, there is nothing more obvious than the groups of movements of some of the elementary geometric figures, such as the regular polygons. We now sometimes emphasize these groups even in elementary mathematics, as may be seen from the useful article on the elements of the theory of groups in the *Enciclopedia delle Matematiche Elementari*.[4]

It is difficult for me to believe that these groups were not observed by some of the ancients. It seems much more probable that they were actually observed by many of them but were regarded as too obvious and unimportant to deserve special mention. At least, no mention even of special cases thereof seems to appear in the mathematical literature before the latter half of the

[2] *American Mathematical Monthly*, vol. 38, p. 94.

[3] *Cf.* G. A. Miller, *Annals of Mathematics*, Vol. 19 (1917), p. 44.

[4] Vol. 1, part 2 (1932), p. 63.

eighteenth century. The rapid progress of mathematics during the nineteenth and the beginning of the twentieth century is largely due to explicit presentations of the obvious in view of the contacts with broad theories which were then rapidly developed. It has recently been discovered that even the ancient Babylonians solved certain quadratic equations by completing the square just as we do now, but no mention of the fact that by completing the square we can always formally solve such an equation has as yet been found in their literature. They did not emphasize such obvious facts since they appeared to them to be isolated.

A characteristic feature of mathematics, and perhaps of all the sciences, is that it has been developed largely as a result of a quest for the comparatively simple but comprehensive. Insurmountable difficulties presented themselves at many points. In number theory, for instance, there remains the question whether the sum of the nth powers of two rational numbers can be the nth power of such a number when $n > 2$, in group theory we have not yet been able to determine whether there is a simple group of odd composite order, in geometry there is the unsolved four color problem, etc. Many such problems might have blocked mathematical advance if the mathematicians had not turned their attention to the easier problems and developed general theories relating thereto which embrace many well-known special results. Abstract group theory, in particular, is such a theory based on such special observations as the following:

The mathematical world became acquainted early with combinations of two elements of a given set of elements such that the result thereof is an element of the same set. For instance, the sum and the product of two numbers are numbers, a geometric movement followed by a second such movement is equivalent to a single geometric movement, the sum of two vectors is a vector, etc. Such combinations are very common, but they are not universal. A pie is quite distinc from the ingredients which enter therei and group theory did not originate in a bakery, notwithstanding the usefulness of this institution. It had its origin in the sphere of human experience whic led the Biblical sage to remark "Ther is nothing new under the sun," notwitl standing the many results commonly accepted as new theorems which appeared in its development.

It may be desirable to refer here to a very unsatisfactory situation in regard to the use of the technical term group in the modern mathematical literature. This may be illustrated by referring to an introductory observation relating to this subject which appears in the part of the recent Italian encyclopedia of elementary mathematics noted above. It is here stated (page 20) that the notion of group has been greatly extended during the last fifty years by including therein operations or transformations whose number may be finite or infinite, whenever the transformation resulting from any two of them, applied successively, is equivalent to a single one of them. The natural inference therefrom is that such vague general notions have been enriched during the last fifty years by the extensive developments of group theory, while, as a matter of fact, not a single theorem thereof applies to them in their complete generality. Hence the statement is apt to be misleading.

It would be better to say that the notion of group is restricted by postulates and hence it can never be extended. A modification of these postulates is, of course, always in order and this may be what S. Lie had in mind when he asserted in his "Theorie der Transforma-

tionsgruppen"[5] that there are groups which involve neither the identity nor the inverse of each of their elements. The confusion which exists to-day along this line is exhibited by the fact that one can now find good recent authority for saying that the natural numbers when combined by multiplication form a group as well as for saying that they do not have this property. Fortunately, many writers state their postulates explicitly and hence avoid all such misunderstanding. One of the most satisfactory systems of group postulates asserts explicitly that only two elements can be combined at a time and that when more than two elements are combined successively they obey the associative law. Moreover, when any two of the symbols of the equation $xy=z$ are replaced by the same element or by two distinct elements of a group the equation will determine uniquely a third element thereof.

It is a somewhat remarkable fact that this simple formulation of the postulates of a group, which applies to those of infinite as well as to those of finite order, has not been more generally adopted by modern writers on this subject since it is briefer than others which are more frequently used and is in line with the fundamental tendency in mathematics towards brevity and comprehensiveness of expression. The term group is appropriate for a comparatively new subject since it is the only technical mathematical term for a large field whose etymology has not been traced back more than 400 years. It seems to have had a common origin in central Europe with the word crop and to have been first used in English in connection with decorations. The nature of the subject would be more clearly exhibited by the term combination which was employed earlier (about 1770) by J. L. Lagrange for the same subject. Perhaps the gregarious nature of the human race led to the adoption of the former term, which was first used in this connection by E. Galois (1811–1832). In fact, in its non-technical sense the group may have exerted a more powerful influence on the development of the number concept than the series.

A conspicuous feature of modern mathematics is the tendency of giving credit for the various decided advances to those who are supposed to have first exhibited them. For instance, in the two parts of the Italian encyclopedia on elementary mathematics which have appeared recently there are author indexes but no subject indexes. The custom of giving such credit has not always been followed. For instance, not a single historical note appears in the famous "Elements" of Euclid and we have no evidence to support the view that even a single mathematical theorem known before the times of the ancient Greeks was then credited to its author. The ancient Greeks inaugurated the credit giving custom in mathematics but not all of them practised it as has been noted in regard to Euclid. It represents an effort to create a lasting memorial to those who have enriched our intellectual lives and it is to be hoped that it will tend to attract more workers into the intellectual fields, which seem to be able to use indefinitely the growing numbers of workers who are no longer needed to provide our physical wants, in view of the growing efficiency of machinery.

The libraries and the laboratories of the world are peace centers not only because they provide enlightenment but also because they provide fields for lasting achievements even for the most ambitious. An organization like the American Sigma Xi aims to attract young university people into this field by acquainting them with some of the work-

[5] Vol. 1 (1888), p. 163.

ers therein and exposing them to the contagion of the enthusiasm of these workers. While the mathematical investigators do not constitute the largest group of scientific workers, more than 3,000 contribute now annually about 5,000 memoirs of sufficient importance to be recorded in the *Jahrbuch über die Fortschritte der Mathematik*, which has recorded such annual advances for more than sixty years. The first volume of this publication recorded less than 500 names of contributors to the advancement of our subject so that the number of mathematical contributors to-day is about six times as large as it was sixty years ago. For America the ratio of this increase is much larger since only four or five American contributors are noted in this first volume, while now this number is several hundred annually. Asaph Hall and Simon Newcomb are the most favorably known Americans whose contributions are mentioned in the said first volume. They are now best known on account of their astronomical achievements.

Mathematics is largely a science of equivalent variants. From the earliest times geometry and analysis have largely supported each other and each has provided a field of interpretation for results obtained in the other. Equivalent rational numbers presented themselves early in an infinite number of forms as common fractions and the restriction of constructions by ruler and compasses in Euclid's "Elements" corresponds to the limitation of finding only the real roots of real quadratic equations in analysis. The conjugates of a group are equivalent variants of each other and similar figures are equivalent under a certain group of transformations. By the principle of duality (which was cleared up in the early part of the nineteenth century by the discussions by J. D. Gergonne, J. V. Poncelet, and J. Plücker) various geometric theorems of different aspects are deduced from each other, and our common space may be regarded either as a point space or as a space of sets of numbers.

Up to the times of the ancient Greeks mathematics was essentially a consideration of relations between rational numbers. The theorems of geometry which were then considered relate to areas and volumes, and hence they were of a numerical nature. The ancient Greeks added thereto the study of geometric properties which were not of a numerical nature, such as the drawing of tangents to curves, but mathematics was confined to the study of numerical relations and geometric figures up to the time when the properties of groups began to be studied in the latter part of the eighteenth century. Group theory is not confined to the study of numerical relations or geometric figures even if it is illustrated by both of these subjects and has greatly enriched them. Elements had been combined since the beginning of mathematics but a special study of the laws of these combinations, even under the very strong restrictions imposed by group theory, marks a new line of development in mathematics, the wisdom of which is substantiated by the rapid developments of this subject in recent years and the numerous applications of the results obtained therein.

The permutations which were studied before the development of group theory were of a numerical nature. They are still studied in our courses in elementary algebra and should not be confused with the permutations of group theory. The latter are often called substitutions to distinguish them from the former. This is still done in the recent Italian encyclopedia of elementary mathematics, to which we referred above, and has many advantages. The permutations or substitutions of group theory are dynamic.

while the permutations of elementary algebra are static. A. L. Cauchy used already the two terms permutations and substitutions in the former sense but he used the latter more frequently than the former and his example was commonly followed by later writers up to the time of H. Weber (1842–1913), who adopted the former term, and his popular algebras have been very influential in turning the tide in favor of this term at the present time. It remains to be seen whether this tide will return.

It should be emphasized that the extensive developments of abstract group theory are largely due to the great restrictions imposed on the combinations considered therein. While these restrictions are arbitrary in a certain sense it would be very far from the truth to assume that they were not suggested by concrete cases. Mathematics is largely the outgrowth of experiments and its abstract theories were often suggested by mathematical physics and were usually adopted very slowly in order to avoid endless repetitions or to simplify the developments. When the ancient Greeks assumed the Archimedian postulate and the closely related postulate that all right angles are equal to each other they probably did so to avoid the necessity of considering certain difficulties rather than to be in closer contact with actual facts. The assumption that curves intersect at the same angle as their tangents at their common points is a remarkable source of mathematical progress posterior to Euclid. If the simple mathematical considerations have led to such an extensive theory it is difficult to predict the scope our subject will assume when we pursue the study of the more difficult and less restricted ones.

In closing we desire to refer to a situation which concerns the professional mathematician less than those who are mainly interested in a bird's-eye view of this subject. The former incidentally secure a penetrating insight into the history of certain fields of mathematics and frequently pay little attention to the general histories of our subject in view of the fact that these histories contain too little relating to their special fields of study and this little is often in an unsatisfactory form. The professional mathematical historians have usually at best either familiarized themselves with some of the ancient or medieval manuscripts relating to very elementary developments of mathematics or culled from widely different sources the views of experts in various fields. The result is that the recent general histories of mathematics usually involve very flagrant shortcomings which could probably be most effectively reduced by a frank discussion thereof in the periodicals. The fact that this method has not always been available without friction results from an announcement in the preface of volume 2 of the oldest extant German mathematical periodical, as follows: "Everything that could injure someone will be unconditionally and rigorously excluded." Even in mathematical publications the truth has not always been paramount and probably never will be even if the Bible says "The truth shall make you free." Possibly the noted policy tends to explain the survival during more than a century of this particular mathematical periodical.

Those who recognize the large number of errors in some of our mathematical histories and are pained by the fact that so many young students as well as laymen are exposed thereto may find some consolation in the observation that most people absorb much more than they digest and what is not digested might almost as well be false as true. The trouble arises when we try to digest

falsehoods relating to scientific matters. The resulting pangs are usually not experienced until after the college days and even then they relate to only a small part of what has been absorbed. Most of us go through life with an undue respect for the printed page and the hope for improvement by evolution rather than by revolution. The laboratories and the mathematical demonstrations have rendered a great service towards freeing intellectual centers from some of their superstitions, but even in these centers the safe truths have as yet to be selected with care and the limits within which some others may be used safely constitute questions of tact.

Various efforts have been made recently to define the term mathematics, but these have naturally been no more successful than the description of an individual while it is still developing rapidly. One could perhaps convey a fairly accurate view by saying that Euclid's "Elements" represent ancient mathematics and that the large German mathematical encyclopedia now represents also modern mathematics, and one does not yet know what future mathematics will be. It is true that such a definition is of little value to those who desire to secure knowledge without personal effort, but to these we can only say that real knowledge usually does not come without such efforts. A large part of the rapid and somewhat burdensome growth of the modern scientific literature is due to an effort to construct automobile roads into the various fields of science, but many interesting details are apt to be overlooked in traveling these roads at the usual automobile speed. This is, however, characteristic of modern science.

THE WORK OF THE BUREAU OF STANDARDS IN ELECTRICITY AND RADIO

By E. C. CRITTENDEN

CHIEF OF ELECTRICAL DIVISION, BUREAU OF STANDARDS

ELECTRICITY provides facilities essential to the modern social organism. Those facilities are created and maintained by a complex and highly organized industry, in which scientific control of product and scientific research looking to further development are most actively employed. In such control and development, precise and reliable measurements are necessary at every step. For workers in every exact science, electrical measurements have a double importance, because electricity not only provides light and power and communication, but also gives the most effective instruments for measurements of manifold kinds. To facilitate these measurements in science and in industry is the primary purpose of the electrical work done by the Bureau of Standards.

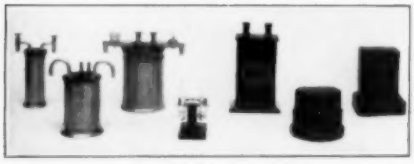
The Bureau as it exists to-day is, however, the result of 30 years of growth and adaptation to the needs and demands of the times, and its work takes a variety of forms which do not fit completely into any simple and logical classification. The various uses of electricity involve different kinds of service given by the Bureau, and its own activities are correspondingly diverse. It is possible here only to mention the more important of these activities and to give a few specific examples illustrating the work done.

THE FOUNDATION OF ELECTRICAL MEASUREMENTS

Electrical measurements are based primarily upon mechanical forces. The methods used for deriving and defining the electrical units have varied, but the general intention has been to make the unit of electrical energy the same as that of mechanical energy, and in effect to measure electricity by the mechanical force exerted upon wires carrying electric currents in a magnetic field.

It is relatively easy to compare one electrical quantity with another, and such measurements can be made with a very high precision; it is, however, much more difficult to establish the correct values for electrical units in relation to the fundamental mechanical effects. Furthermore, the necessity of having uniform electrical units throughout the world has long been recognized because electrical measurements play so important a part in the world-wide interchange of scientific data as well as of industrial products. A series of congresses and conferences extending over a period of 30 years (from 1881 to 1910) therefore developed our present "international" system of units, which was intended to provide a uniform basis for electrical measurements approximately consistent with the mechanical units. This system of electrical units starts with an ohm, which is the resistance at 0° C. of a uniform thread of pure mercury 106.3 cm long and weighing 14.4521 grams, and an ampere, defined by specifying that it will deposit 1.118 milligrams of silver per second. In practise, however, the basic standards are wire resistance coils for the ohm and standard cells of prescribed electromotive force, and international agreement is maintained by comparisons of such standards.

The Bureau of Standards was created in 1901; for the first decade of its existence its electrical research work was largely directed to the establishment of



PRACTICAL STANDARDS OF RESISTANCE AND OF ELECTROMOTIVE FORCE

RESISTANCE COILS AND STANDARD CELLS OF THE TYPES SHOWN CONSTITUTE THE BASIS OF ALL PRECISE ELECTRICAL MEASUREMENTS. COILS ARE MOUNTED IN AIRTIGHT CASES; THE THREE SHOWN AT THE LEFT REPRESENT SUCCESSIVE STEPS IN DEVELOPMENT MADE AT THE BUREAU OF STANDARDS. THE SMALL CELL IN THE CENTER IS A SATURATED OR WESTON NORMAL CELL OF THE TYPE USED BY THE NATIONAL STANDARDIZING LABORATORIES TO MAINTAIN THE UNIT OF ELECTROMOTIVE FORCE, THE VOLT. THREE STANDARD CELLS OF THE UNSATURATED, PORTABLE TYPE, AS SUPPLIED COMMERCIALLY, ARE SHOWN AT THE RIGHT.

satisfactory basic units and the physical standards needed to maintain them. Under the leadership of Dr. Edward B. Rosa this experimental work rivaled that of the best European laboratories and gained such recognition that the final steps in the establishment of the international units were carried out at the Bureau by a committee including representatives from France, Germany and Great Britain. The system thus set up was put into use in 1912 and is still maintained through the cooperation of national laboratories in the larger countries. In this work the Bureau of Standards has taken a leading part; through the care exercised in the choice and use of its reference standards and the provision of larger numbers of them it has been more successful than any of the other laboratories in preserving stable values of the units.

With the establishment of such an international system of units on a basis presumed to be permanent this problem seemed to be settled; and other needs of more immediate urgency displaced further work on fundamental units for many years. It was, however, already known that the "international" electrical units were not so close to the true or "absolute" units as was originally supposed; the developments of modern physics and chemistry showed continually closer coordination of electrical and mechanical phenomena, and the necessity for using correction factors when the two were compared threatened to cause confusion in the future. Furthermore, increasing differences were found between the units used in different countries, while makers and users of precision apparatus were continually calling for greater and greater accuracy. It became evident, therefore, that the fundamental basis of electrical measurements must be reconsidered.

In the meantime, by an amendment to the treaty under which the International Committee and the International Bureau of Weights and Measures operate, those organizations were given jurisdiction over electrical units. An Advisory Committee on Electricity was formed in 1928, including the Bureau of Standards

DIAGRAM SHOWING PRINCIPLE OF THE RAYLEIGH CURRENT BALANCE

AN ELECTRIC CURRENT FLOWING THROUGH THE COILS C, D AND E IS MEASURED BY WEIGHTS BALANCING THE ELECTROMAGNETIC FORCE EXERTED ON C.

THE CURRENT BALANCE AS ACTUALLY USED

THE TWO FIXED COILS CAN BE SEEN IN THE LOWER CASE. THE MOVING COIL SUSPENDED FROM THE BALANCE ARM IS ENCLOSED. WEIGHINGS ARE MADE BY AN OBSERVER OUTSIDE THE ROOM THROUGH THE WINDOW AT THE LEFT.

and national laboratories of France, Germany, Great Britain, Japan and the U. S. S. R. (Russia). This committee decided unanimously that the electrical units should be revised as soon as new determinations of their absolute values could be made with sufficient certainty. Since that time the precise establishment of the ohm and ampere by absolute measurements has been one of the major research projects of the Bureau.

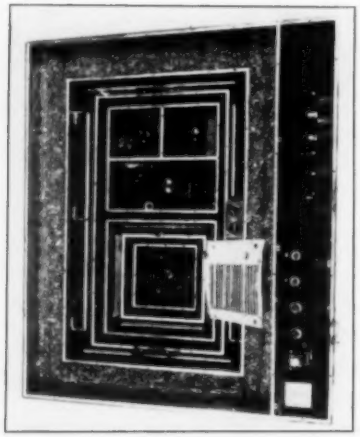
A determination of each unit by two different methods was planned. One determination of each unit is now nearly completed. Other laboratories are also making such measurements, and it is hoped to reach at least a preliminary international agreement at a meeting to be held in 1935. If possible, the new units will then be put into use in 1937.

The principle applied in the absolute measurement of current is simple. Two parallel wires carrying current attract or repel each other. If the two wires are wound into coils of many turns, and one coil is hung from the arm of a balance, while the other is held above or below and parallel to the first, the electromagnetic force between the coils can be directly measured as an apparent change in the weight of the coil on the balance. A roughly approximate measurement of current can thus be made very readily, but when the final results must be correct to one or two thousandths of one per cent, innumerable difficulties arise. For example, the forces to be measured are only a few grams weight superposed on the weight of the coil. The actual weighings must therefore not have an error as great as one in ten millions; for such weighing uniformity of temperature about the balance is ordinarily considered essential, but in this case the current-carrying coils constitute a good electric heater which is necessarily placed near the balance.

The force also depends on the length of wire in the coils and on the effective distance between the coils. A direct determination of these quantities with the necessary precision would be almost impossible; this particular obstacle is

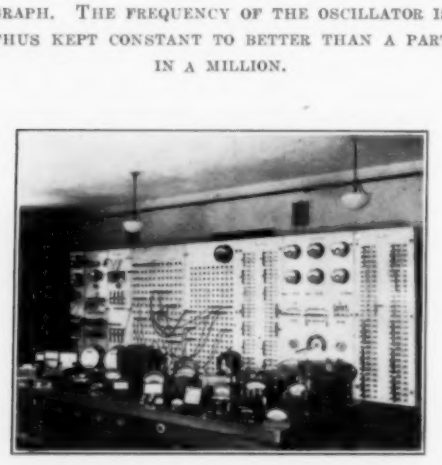
INDUCTANCE COIL USED IN DETERMINING THE MAGNITUDE OF THE OHM

THE COIL IS A SINGLE-LAYER WINDING ON A CAREFULLY PREPARED CYLINDER. ITS DIMENSIONS ARE MEASURED BY AN ELECTRICALLY CONTROLLED MICROMETER GAGE OPERATING INSIDE THE HEAT-INSULATED CHAMBER (SHOWN OPEN IN THE PHOTOGRAPH).



PIEZO-OSCILLATOR DEVELOPED BY BUREAU OF STANDARDS AS SEMI-PORTABLE STANDARD OF FREQUENCY

A QUARTZ PLATE OSCILLATING AT 100,000 CYCLES PER SECOND IS MAINTAINED AT CONSTANT TEMPERATURE AND BAROMETRIC PRESSURE. A TRIPLE TEMPERATURE CONTROL IS USED ABOUT THE NEST OF ENCLOSURES SHOWN OPEN IN THE PHOTOGRAPH. THE FREQUENCY OF THE OSCILLATOR IS THUS KEPT CONSTANT TO BETTER THAN A PART IN A MILLION.



ELECTRICAL INSTRUMENTS TESTED BY THE BUREAU OF STANDARDS

A TYPICAL COLLECTION OF INSTRUMENTS SUBMITTED FOR DETERMINATION OF THE ACCURACY OF THEIR CALIBRATIONS. IN THE BACKGROUND IS THE SWITCHBOARD THROUGH WHICH THE INSTRUMENT LABORATORY IS SUPPLIED WITH CURRENTS OF DIFFERENT VOLTAGE, FREQUENCY AND WAVE-FORM.

avoided by using two fixed coils between which the weighed coil is hung in such a way that only the relative sizes of the three coils need be known precisely. Their relative dimensions are determined by a separate series of measurements on the magnetic fields produced by the coils, but these measurements are so delicate that they can be made only when there are no magnetic disturbances. It has been possible to make them only by working after the street cars had stopped at night, and then only on nights when the earth's magnetic field was comparatively constant.

The difficulties cited are only a few of the many which require continual vigilance in such work. One by one these difficulties are overcome, but years are required to attain the necessary certainty in the final result.

The method which has been used to fix the absolute value of the ohm consists of several steps. First a single helix of wire with very uniform spacing is wound on a form which has been ground to be as perfect a cylinder as is practicable. Then the exact dimensions of the coil thus produced are measured, and from them the inductance of the coil is calculated in absolute units. The values of inductance thus found are carried over into units of resistance through measurements of a condenser, which is measured first with the inductance coil and then with standard resistance coils. The work thus summarized in a brief paragraph has required several years since special apparatus has had to be devised at every step and new formulas have had to be developed for the calculations.

While this experimental work on the fundamental units has been in progress, improvements have also been made in the practical standards which serve to maintain the units, that is, resistance coils and standard cells. A new type of standard resistor has been devised which has shown variations less than one tenth

of those to be expected with the best previous standards. Three such coils have made three trips to the European national laboratories during the past five years, and none of them has apparently changed more than two parts in a million. This type of coil therefore provides a highly dependable means for transferring values from one standardizing laboratory to another and for checking the instruments used in practical measurements.

In parallel with these laboratory developments the Bureau has carried on negotiations with the corresponding authorities abroad, and has developed in advance a plan for prompt establishment and dissemination of the new units as soon as the determinations now in progress here and abroad are completed. Provision has also been made for periodic comparisons of standards through the International Bureau of Weights and Measures, so that differences between the units of different countries will be detected and remedied. Incidentally this will, of course, give double assurance of the accuracy of our own standards.

Electrical Standards for Practical Use

The establishment and maintenance of correct fundamental standards would be of little use without some provision for carrying over into practical measurements the units which they preserve; the provision of practical working standards is therefore a second essential function of the Bureau of Standards. Every electrical measuring instrument made in this country is calibrated by reference to standards certified by the Bureau; practically every meter by which electricity is sold in home or factory is not only thus calibrated originally by the maker, but is also retested periodically by the power company or the public service commission, both of which come back to the Bureau for certification of their reference standards; electric lamps of standard quality all go through a process of inspection involving continual tests with instruments kept in close agreement with the Bureau's standards; every important research and testing laboratory in this country depends upon the Bureau for verification of its electrical standards.

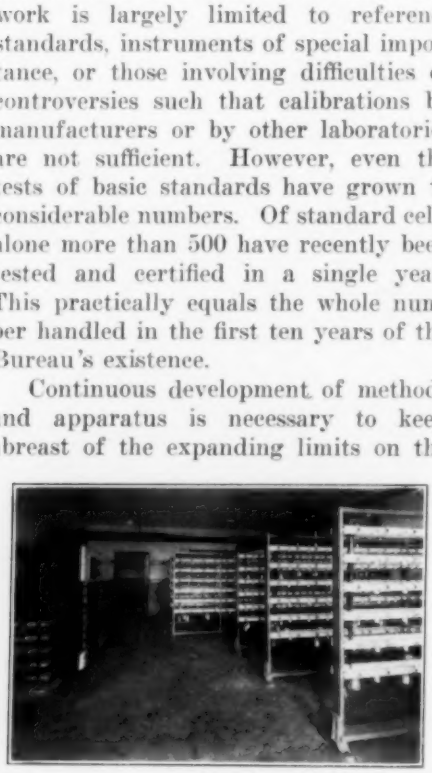
The starting point for all these measurements is the standard cell and the coil of one ohm resistance, but there are often many steps before the final result is reached. Precise resistance standards, for example, must cover the range from one thousandth of an ohm up to 100,000; potentials must be measured accurately from millivolts to hundreds of thousands of volts and at least estimated further out in the scale in microvolts or in millions; measurements must be made with alternating currents as well as di-

AN INSTRUMENT TOO LARGE FOR THE LABORATORY

The transformer shown was intended for use as a standard for measurements on a 132,000-volt system in California. Since the Bureau's high-voltage laboratory was too small to accommodate it, the transformer was tested by bringing connections out as shown.

rect; inductance, capacitance and other effects arising from changing values of the current must be measured. The development of instruments suitable for all these measurements is, of course, a commercial problem, but besides furnishing the basic standards the Bureau is expected to be able to verify the final results. It receives, therefore, a great variety of instruments for test; the magnitude of this work arises from variety rather than number of instruments because the Bureau does not test electrical instruments on a wholesale basis. The work is largely limited to reference standards, instruments of special importance, or those involving difficulties or controversies such that calibrations by manufacturers or by other laboratories are not sufficient. However, even the tests of basic standards have grown to considerable numbers. Of standard cells alone more than 500 have recently been tested and certified in a single year. This practically equals the whole number handled in the first ten years of the Bureau's existence.

Continuous development of methods and apparatus is necessary to keep abreast of the expanding limits on the use of electricity in industry. When electricity is transported and delivered at 220,000 volts, its accurate measurement requires methods quite different from those used at low voltages of ordinary power supply. To assure the accuracy of its measurements at high voltage the Bureau is developing two independent methods of getting the results. The first is to start from ordinary voltages and build up to the high ones through combinations of standard transformers whose characteristics are carefully studied and whose circuits are designed to avoid errors due to electrical interaction between parts of the circuit and surrounding walls or other objects. The second method is to go back to fundamental principles and actually to weigh the force of attraction between disks charged up to the voltage which is to be measured. An absolute electrometer has been built for this purpose. Measurements with it are limited at present to 150,000 volts by the dimensions of the laboratory, but the electrometer will be capable of measuring up to 350,000 volts when sufficient space around it can be provided.



LIFE-TESTS OF ELECTRIC LAMPS

ON THE RACKS SHOWN LAMPS ARE BURNED TO DETERMINE WHETHER THEY GIVE THE LIFE AND THE EFFICIENCY REQUIRED BY THE SPECIFICATIONS UNDER WHICH THEY ARE PURCHASED BY THE GOVERNMENT. FROM 3,000 TO 4,000 SAMPLE LAMPS ARE THUS BURNED EACH YEAR; THESE SAMPLES ARE SELECTED FROM PURCHASES OF TWO TO THREE MILLION LAMPS.

In the measurement of large electric currents also, it has been necessary to devise special instruments in order to avoid errors arising from the large heating effects and the strong magnetic fields caused by such currents. The Bureau is now prepared to calibrate instruments up to 4,000 amperes on direct current and to 12,000 amperes alternating.

DEVELOPMENT OF SPECIAL INSTRUMENTS

While development of standards and special instruments is carried on primarily to meet the Bureau's own needs, a very important by-product is the improvement of apparatus for other purposes. For example, various types of resistance and inductance coils, galvanometers, potentiometers, transformer testing sets and magnetic testing appa-

ratus devised at the Bureau are widely used, and further improvements in such measuring apparatus are continually made available through cooperation with manufacturers of instruments.

More spectacular achievements may be cited in many cases where the experience of the Bureau's staff has been applied in fields outside its own regular work. For example, electrical instruments designed and built at the bureau to record the movements of heavy guns and the deformation of gun turrets have provided the basis for present design of such structures. Electrical gauges developed by the Bureau serve to record the strains in bridge members under live load, in mine hoisting ropes, in stays and structural members of airplanes and airships in flight and in the interior of massive concrete dams. The ships of the International Ice Patrol measure the salinity of the sea with electrical instruments designed and made at the Bureau of Standards. Studies of electromagnetic instruments at the Bureau have produced the best available seismometers for recording distant earthquakes, as well as the only instrument capable of giving a time record of the disturbances close to an earthquake center. Three of these were located in Long Beach and Los Angeles, and the records which they gave of the quake of March 10, 1933, provide a basis hitherto lacking for the design of buildings in such regions.

EQUIPMENT FOR TESTS OF DRY CELLS

TESTS OF DRY CELLS MUST SIMULATE SERVICE CONDITIONS, INCLUDING SPECIFIED PERIODS OF DISCHARGE WITH INTERVALS FOR RECUPERATION. APPARATUS DEVELOPED AT THE BUREAU CARRIES OUT THESE DISCHARGES AUTOMATICALLY ON HUNDREDS OF CELLS. THIS PARTIAL VIEW OF THE LABORATORY SHOWS RACKS OF CELLS AT THE LEFT, COUNTERS FOR RECORDING NUMBER OF DISCHARGES IN CENTER, AND A TELEPHONE SWITCHBOARD THROUGH WHICH INSTRUMENTS ARE EXPEDITIOUSLY CONNECTED TO THE VARIOUS CELLS WHEN MEASUREMENTS ON THEM ARE DUE.

Properties of Materials

The electrical industry is particularly concerned with materials of three kinds; conductors, insulators and magnetic materials. Practically the only conductors used in large quantities are copper and aluminum. The Bureau has made exhaustive investigations of the properties of these materials, and has issued various compilations of data on them. For copper as commercially supplied a standard value of conductivity based largely upon the Bureau's results was adopted some years ago by the International Electrochemical Commission and is used throughout the world. With regard to aluminum, negotiations for a world-wide standard have not been concluded, but a standard conductivity based on the bureau's values has been adopted by the American Institute of Electrical Engineers and approved by the American Standards Association for use in this country. Conductivity of materials and methods of measuring it are now so well standardized that the chief service required of the Bureau is to check the accuracy of tests made in commercial practise. This is done, on request, by making precise measurements on standard rods or sample wires for comparison with the results obtained at the industrial plant or testing laboratory.

Magnetic materials (iron and steel) are not susceptible of standardization in the sense that conductors are, because they vary widely in composition and because their properties are affected so much by heat treatment and mechanical

USE OF THE EARTH-CURRENT METER

THIS INSTRUMENT DEVELOPED BY THE BUREAU OF STANDARDS MEASURES EITHER ELECTRICAL RESISTIVITY OF SOIL OR ELECTRICAL CURRENT FLOWING IN THE EARTH, AS, FOR EXAMPLE, FROM A PIPE SUBJECT TO DAMAGE BY ELECTROLYSIS. EXCAVATION AROUND THE PIPE IS NOT NECESSARY, SINCE THE MEASURING ELECTRODE CAN BE INSERTED INTO A DRIVEN HOLE AS SHOWN.

working. The Bureau's researches on these materials have therefore been largely directed toward the establishment of reliable methods for determining the magnetic characteristics of a given sample of material. Through cooperation with the American Society for Testing Materials various standard methods have been established and in this case also tests are made by the Bureau on request as checks on measurements made in commercial laboratories.

Measurements of the magnetic behavior of iron or steel sometimes gives important information about its mechanical properties. For example, forgings may thus be rapidly inspected for uniformity and freedom from flaws, but the relations between magnetic and mechanical properties are not simple. The Bureau has carried on a large part of the scientific work done to establish the applicability of magnetic testing methods for such purposes.

Electrical insulating materials are even more diverse and uncertain in character than magnetic materials. They include solids, liquids and even gases; and the properties desired depend largely on the place and the purpose for which the insulator is intended. The Bureau's work on insulators is largely research directed toward finding out why the materials behave as they do, since this is the first step toward making them better. This research at present consists chiefly of a study of rubber and rubber compounds and of very pure liquid insulators.

On electric lamps and batteries the Bureau regularly makes comprehensive tests, primarily to assure the quality of materials supplied on Government purchases. These tests include both efficiency or output of the devices and their life. They serve not only to determine the acceptability of materials supplied, but also to give information needed in periodic revisions of the Government specifications for these items. Incidentally also they have an important influence in promoting competition between makers on the basis of efficiency and real value of the product.

Engineering Research and Applications

In three distinct fields the Bureau of Standards has carried on a considerable amount of electrical work of an engineering type of which only the briefest mention can be made here. The Bureau's study of electrolysis by stray electric currents in the earth has resulted in improved methods and instruments for making electrolysis surveys, and its publications constitute the standard reference books on this subject; but, as a contribution even more important economically, it established the principle of settling such complex technical problems by cooperative scientific and engineering research rather than by legal arguments in the courts. Out of these studies also grew experimental tests of the corrosion of pipe materials in typical soils in all parts of the country, methods of predicting where corrosion

would be serious, and tests of protective coatings to reduce the loss due to corrosion. On account of the rapid growth in long pipe lines in recent years this general problem of corrosion in soils has overshadowed completely the special case of electrolysis.

Telephone service is an exceedingly important item in a business as large as the Federal Government; in recent years all important problems of telephone service in all departments have been referred to the Bureau of Standards for advice. It has been necessary not only to decide how to furnish service most economically and effectively in hundreds of new public buildings throughout the country, but also to choose special equipment for hospitals, prisons and other institutions. The net result has been that the telephone engineering group of the Bureau has spent all its time on these government problems, but the amounts directly saved to the Government thereby have been many times the cost of this service.

The third type of engineering activity is the development of codes of practise in industry, especially those affecting safety of persons. The Bureau took a leading part in organizing this work on a national scale under the procedure of the American Standards Association; this gives the authority and responsibility for all decisions on technical questions to committees representing all organized interests concerned in the code. The outstanding example of the Bureau's work in this field is the National Electrical Safety Code, a very complete set of rules to be observed in the generation, distribution and use of electric power, in order to make the service as free as possible from hazards to persons.

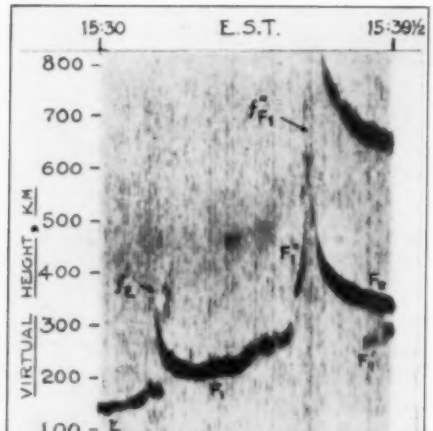
Radio Measurements and Standards

Radio communication is simply an application of oscillating electric currents and electric fields, but their rapid oscillation introduces new problems of measurement which have become more and more difficult as the range of frequencies used has been pushed upward. In mechanics, Newton's laws seemed adequate and exact as long as one dealt with velocities of moderate value, but in the newer physics the behavior of particles moving with much greater speeds has given rise to a new and more complicated science of mechanics. Similarly, radio has its own technique of measurement in which effects negligible in ordinary electrical measurements become the most important factors.

A large part of the Bureau's work in radio has been intended to supply to this new industry the necessary methods of measurement and information on the behavior of devices and materials in rapidly alternating electric fields. Methods

RECORDERS DESIGNED BY BUREAU OF STANDARDS TO MEASURE THE INTENSITY OF RECEIVED RADIO WAVES

THREE RECEIVING SETS WITH AUXILIARY APPARATUS AND RECORDING MECHANISM MAKING CONTINUOUS RECORD PROVIDE DATA FOR STUDY OF RADIO FADING, INTENSITY VARIATIONS, ATMOSPHERIC DISTURBANCES AND CORRELATIONS WITH SOLAR AND MAGNETIC PHENOMENA.



CHARACTERISTICS OF IONIZED LAYERS OF UPPER ATMOSPHERE DETERMINED BY RADIO MEASUREMENTS

RADIO SIGNALS ARE REFLECTED BACK TO EARTH BY THESE IONIZED LAYERS. AN AUTOMATIC APPARATUS DEVELOPED BY THE BUREAU OF STANDARDS RECORDS THE LAYER HEIGHT AS A FUNCTION OF FREQUENCY. THREE DISTINCT LAYERS ARE ILLUSTRATED, CALLED THE E, F_1 AND F_2 LAYERS. FROM THE CRITICAL FREQUENCIES, f_E AND f_{F_1}, AT WHICH A CHANGE IS SHOWN FROM ONE LAYER TO ANOTHER, THE IONIZATION IS CALCULATED.

and instruments have been developed for measuring radio currents and resistances, signal strength and power lost in insulators. Condensers and coils adapted to radio frequencies have been designed. Properties of electron tubes, antennas of various forms and insulating materials have been studied. However, with increasing demands for the use of radio in different services, the most pressing problem has become that of accurate measurement of frequencies, because the effective use of the available radio spectrum depends upon precise control of the frequency used by each station. Consequently the Bureau's work on radio measurements is now largely concentrated on the development and maintenance of exact standards of frequency.

In recent years most remarkable precision has been attained in such standards by using oscillating plates of crystal quartz. In this material, mechanical pressures and electric fields are so related through the piezo-electric effect that the plates can be set into vibration by alternating fields and in turn the natural frequency of vibration of a plate can be made to control the currents in a suitable circuit. Many laboratories have contributed to the development of devices using this principle. By making full use of the work of others and adding refinements, the Bureau has built, and operates continuously, a set of standards which supply radio frequencies accurate to a few parts in 100,000,000. To attain this accuracy four independent piezo-oscillators are used, each in a double thermostated chamber and each supplied with electric drive which is most carefully controlled. Each oscillator is adjusted approximately to 100,000 cycles per second, continuous records are made of the beats arising from differences in frequency of the four oscillators and the actual frequencies are determined by comparison with corrected standard time signals over a considerable period. Frequencies higher and lower than that of the primary standard, and of practically equal accuracy, are furnished continuously by using multiples and submultiples of that frequency.

In order to make its own accurate standards widely useful, while keeping down to reasonable limits the routine testing demanded of it, the Bureau began ten years ago to transmit signals of specified standard frequencies. Such signals are now sent out at regular intervals on a frequency of precisely 5,000,000 cycles per second. A 30-kilo-

watt transmitter installed in April, 1933, is expected to make this service directly usable throughout the United States. Incidentally, it will also make possible direct comparisons with European laboratories, which are important on account of the world-wide effects of some radio transmissions.

Propagation of Radio Waves

The proper distribution of the radio spectrum among different services requires a knowledge of the distance range attainable and the dependability of transmission with different frequencies. Phenomena of transmission also affect vitally such services as radio compasses and radio beacons. A second large division of the Bureau's work is therefore the study of behavior of waves between transmitter and receiver. The results of this work have been of the utmost value in the allocation of frequencies and the spacing of stations both in this country and throughout the world.

It is well known that long-distance radio is possible only because the upper region of the atmosphere is ionized and therefore constitutes an electrical conductor (Kennelly-Heaviside layer) which refracts or reflects waves back to the earth. In recent years, however, it has been discovered that the conducting layers are manifold and complex and are continually changing, so that waves of various frequencies are reflected quite differently at different times. The Bureau of Standards takes a leading part in a systematic study of these effects which is being carried on throughout the world through the International Scientific Radio Union. Instruments have been devised which make continuous records of the heights of the conducting layers which are effective in reflecting waves of various frequencies and of the variations in strength of the signals received from distant stations. Besides their use in the fundamental studies, such instruments find many immediate practical applications, such as observations on broadcasting stations attempting to operate in synchronism.

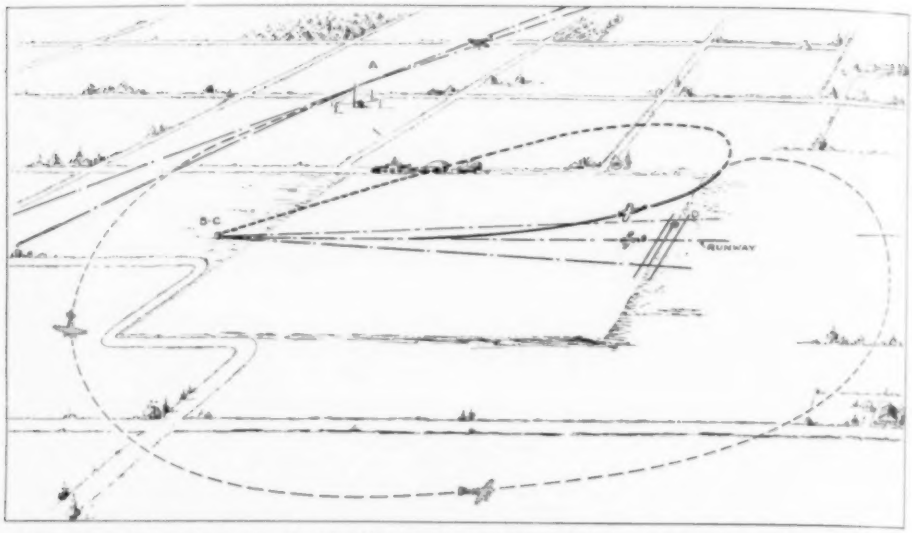
Radio Devices and Applications

While the Bureau's own responsibilities lie in the field of measurement, standards and fundamental research, it is frequently called upon by other Government agencies to devise apparatus for their use. So, for example, when the Air Mail was inaugurated, the Bureau of Standards was called upon to develop radio sets for planes; the Bureau began broadcasting market reports for the Department of Agriculture before commercial broadcasting was established and helped in the development of sets for receiving them; it devised the original models of all the several systems for radio direction finding later used and further developed by the Navy, the Lighthouse Service, the Army Air Service and finally the airways of the Department of Commerce. The latter systems use directive beacons which indicate to a pilot whether he is on or off his course by signals received on an ordinary radio set.

When the Aeronautic Branch of the Department of Commerce was estab-

AIRWAY RADIO BEACON STATION USING TL (TRANSMISSION-LINE) ANTENNA

This recent development of the Bureau of Standards eliminates errors in indicated courses of the airways radio beacons which are caused by horizontally polarized components in the transmitted waves from loop antennas. A special type of transmission line connects the transmitter with each vertical antenna so that no horizontal part of the system radiates.



SKETCH OF BUREAU OF STANDARDS SYSTEM OF RADIO AIDS FOR BLIND LANDING OF AIRPLANES

AFTER BEING GUIDED TO THE VICINITY OF THE LANDING FIELD BY THE RADIO BEACON A, THE PILOT RECEIVES LATERAL GUIDANCE ALONG THE LANDING RUNWAY BY A RUNWAY BEACON B AND VERTICAL GUIDANCE BY THE LANDING BEAM PRODUCED BY A TRANSMITTER C. HE RECEIVES INFORMATION OF LOCATION LONGITUDINALLY WHILE LANDING BY SIGNALS FROM MARKER BEACONS D. THE SYSTEM IS INSTALLED AT THE NEWARK, N. J., AIRPORT.

lished, the Bureau of Standards was designated by law to carry on research for it, and in this connection has developed many improvements in the directive radio beacon. These include instruments to give the pilot a visual indication of his position instead of the signals formerly received only by ear, combined transmitters to keep up simultaneously telephone communication with the same receiving set which serves for the beacon signals, a shielding system to prevent the engine ignition from interfering with radio reception, methods of adjusting the several courses of one beacon so as to serve airways converging at various angles, and special transmitting antennas which greatly reduce the errors in course indications previously experienced at night.

The development most recently carried to a practical stage is a complete system for blind landing of aircraft. Equipment of this type was installed at the Newark (N. J.) Municipal Airport early this year. The practicability of the system has been demonstrated by numerous landings in a hooded cockpit and also under fog conditions which interrupted all scheduled flying over a wide-spread area, including the Newark Airport. The tests included a completely blind flight in dense fog from College Park, Maryland, to Newark Airport during which the ground was entirely hidden until the plane was within about 2,000 feet of Newark Airport at an altitude of somewhat over 100 feet. A brief description of this system of flying aids which makes such flights possible was published in the SCIENTIFIC MONTHLY for July, 1933, p. 94.

HOHOKAM: A CHAPTER IN THE HISTORY OF RED-ON-BUFF CULTURE OF ARIZONA

By CARL TRISCHKA

CHIEF GEOLOGIST, PHELPS DODGE CORPORATION, COPPER QUEEN BRANCH, BISBEE, ARIZONA

INTRODUCTION

WILLIAM MARDON, of Bisbee, Arizona, who has been interested in archeology for a number of years, discovered the campsites in 1929, and since then he has found many things of interest, all of which will be described in this article. He has done a great deal of digging and has unearthed really worth-while finds. To him belongs the credit of discovery.

The writer became interested in the ancient communities in 1930 and took the photos, copied the designs of the pottery, made the sketches of the hearths and has tried to digest the current opinions of archeologists regarding the Hohokam with the idea of applying them to the Red-on-Buff campsites under discussion.

Many of the ideas advanced in this paper are based on observations and deductions of Mr. Mardon and the writer and were formed from evidence seen on the ground. Much work still remains to be done and this will continue as time is found for it. We are indebted to the well-known archeologists, Dean Byron Cummings, of the Arizona State Museum at the University of Arizona at Tucson, and Dr. H. S. Gladwin, of Gila Pueblo near Globe, Arizona, for information, helpful suggestions and encouragement in the work. Mr. Wm. Stevenson kindly gave permission to excavate the campsites which are on his ranch.

The Gila Basin of Arizona, which now is the location of many peaceful agricultural settlements and the scene of industrious mining operations, was at one time the stage of romantic Spanish adventures and gruesome massacres by Apache Indians. However, long before that, men lived here in large communities. They left no written history, but from their handiwork and the tree rings[1] of the poles of their dwellings and the charcoal of their fires, the story is gradually being put together. But even before these times, in the Pleistocene, 20,000 to 30,000 years ago, when now extinct species of the mammoth, the mastodon, the bison and the horse roamed these plains, men lived here, as attested by the finding of an arrow-head in the matrix surrounding the bones of such an animal in near-by New Mexico, and the finding of crude stone implements of human manufacture and use and a fireplace in a stratum 15 feet below the surface, near Bisbee, Arizona. In the same stratum were found bones of extinct species of bison and horse and in a stratum 5½ feet above was uncovered the head of a mammoth.

It is believed that, about 200 B. C., a race of people came up the west coast of Mexico, and in the earlier periods of the culture drifted as far north as the Verde River and Tonto Creek. Later, they became sedentary along the Gila River and its tributaries. These people raised corn and made pottery. The corn was small and quite a different product from the one which present-day Americans raise. They cremated their dead and knew a great deal about irrigation. They were a remarkable people, peaceable, intelligent, and hard workers.

A great drought, lasting from 1276 to 1299, was probably one of the causes that made them move about searching

[1] A. E. Douglass, *National Geographic Magazine*, December, 1929.

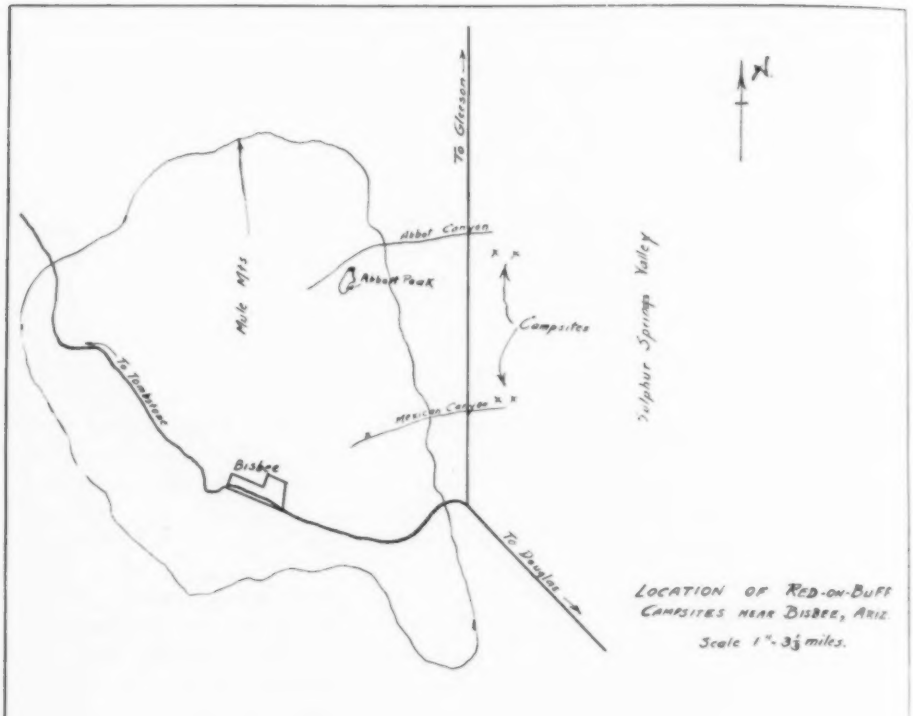


FIG. 1

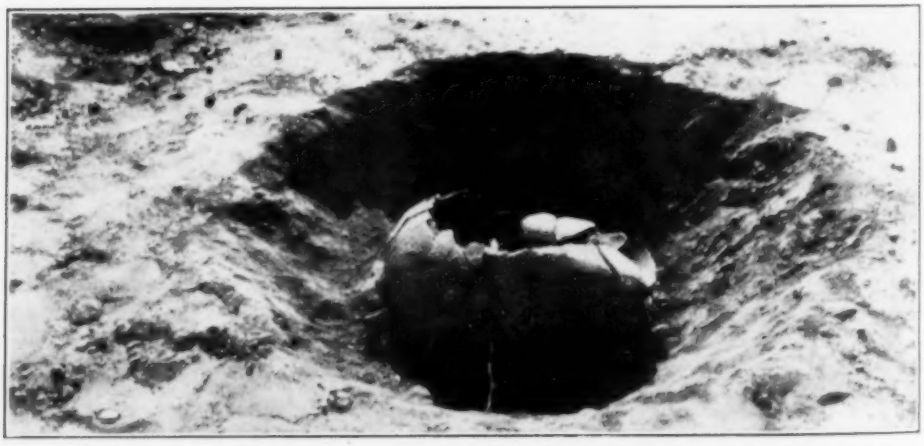


FIG. 2. LARGE BOWL WHICH CONTAINED CHARRED SUNFLOWER SEEDS

for water; and, not finding it in the area of Arizona, they finally left the country and drifted south, eventually, perhaps, becoming one of many tribes that were ruling Mexico when Cortez came to America. Some of them, however, remained, and the Papago and Pima Indians of Arizona may be their descendants. These people called them the Hohokam or "Ancient Ones," and the name "Hohokam" has been recently given them by archeologists.

The most characteristic artifact is their pottery, which they developed through four or five stages from very poor unslipped material to well-slipped, highly decorated and polished work. It seems strange that their product later reverted back again through decadence to a poorer quality. Throughout this development, the background of the decorated pottery was buff and the painting on it red, hence the name "Red-on-Buff."

These people had neighbors of various kinds, some of whom no doubt found these peaceable agriculturists easy victims. To the north lived the Puebloans, who built cliff dwellings and two or more storied pueblos, in which the ladies reigned supreme. To the east were the Mimbres people, whose black-on-white pottery is very attractive and who acted as a barrier against the warlike plains tribes. To the south were various Mexican cultures, about which very little is known, and to the west were the Yumas, who in their early development were more warlike than later.

While these people and most of their neighbors were sedentary, architecturally inclined, and tilled the land, the ancestors of many of us were wild unconquered hordes of rather low order of intelligence who built the great stone monuments of England and formed the Middens on the Danish and Scandinavian coasts. The Hohokam culture, though neolithic, was a remarkable one and one wonders what would have been their fate had they had the advantage of ancient contacts with the far advanced state of civilization to the south, as did our ancestors in their contacts with Rome, Greece and Egypt. Why did they degenerate? What finally became of them?

In Arizona there are three well-equipped and earnest groups of archeologists who are doing much to unravel the now popular puzzle of the ancients

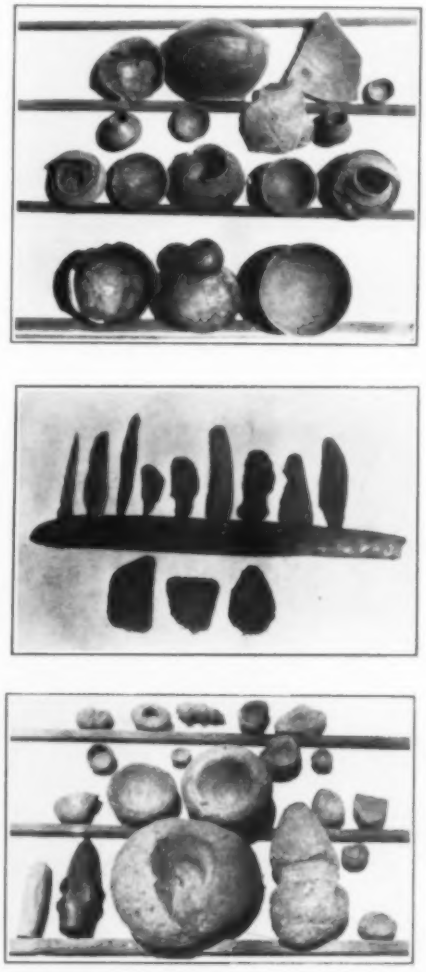


FIG. 3. ARTICLES FOUND IN CAMP SITES
Upper: RED-ON-BUFF POTTERY. *Middle:* STONE PICKS, COMBS, KNIVES AND SCRAPERS. *Lower:* STONE MORTARS AND AXES.

FIG. 4 (a). HEARTH, TYPE 1

SKELETON ON ROCKS. ACCIDENTAL BURIAL VAULT.

FIG. 4 (b). CLOSE-UP OF SKELETON

SHOWING CUPPED HANDS AND DRAWN-UP LEGS.

of the Southwest. They are at the Arizona State Museum at the University of Arizona, Tucson; the Gila Pueblo, near Globe, and the Northern Arizona Museum, at Flagstaff. It is a problem that not only takes the best of talent but demands patient and continued interest. Arizona is fortunate in having these able people to work on its ancient history, and they are making excellent progress.

The pictures shown here are of things made by the Red-on-Buff people in an area eighteen miles long and four miles wide. They came to this country perhaps empty-handed as far as artifacts are concerned, but with ideas about things and some knowledge of the arts. They had corn, squash, sunflower and other seeds, but found in the country many native fruits, such as mesquite beans, piñons and acorns. Of game there probably was plenty, and a living was gotten comparatively easily. They had no metal, but out of and with rock they fashioned everything they needed. Imagine yourself put to the necessity of doing this!

These people were sedentary and soon established themselves wherever there was water for irrigating, good soil for farming, clay for pottery and rocks fit for their uses. There were many such sites in the Southwest. They were soon settled and content, and it can be imagined that they were superstitious and on this account sometimes would change their location on short notice. They had no horses, sheep or other domestic animals, except perhaps dogs and turkeys. Before they made pottery, they made vessels out of various kinds of rock. They were truly a Stone Age people.

Every Red-on-Buff site represents a chapter in the history of the story of their development and only after a great deal of work (actual digging) and research among records of previous finds, as well as a close study of pottery and the remains left in the camps, is it possible to reconstruct a part of the chapter; much will never be known, because perishable artifacts are destroyed, and they left no written history, except the pictographs of the cliffs.

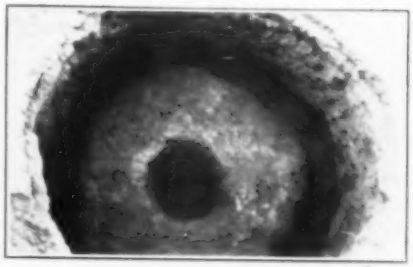


FIG. 4 (c). AFTER REMOVAL OF SKELETON

ROCKS AND ASHES WERE FOUND IN THIS FIRST HEARTH BOTTOM WITH ITS DOUGHNUT-LIKE PATTERN.

FIG. 5. BOTTOM OF A HEARTH

THIS WAS DUG IN THE STREAM OR CREEK BOULDERS. SANDY TOP SOIL MADE A SMOOTH CONTACT WITH THE LAYER OF BOULDERS.

FIG. 6. THE HEARTH WALLS

ON DIGGING ON THE OUTSIDE OF THE WALLS IT WAS FOUND THAT THEY CONSISTED OF 3 TO 4 INCHES OF RED BAKED CLAY OR ADOBE AND THAT THIS CLAY HAD BEEN DAUBED ON AFTER THE HOLE, WHICH WAS OF AN OLLA-LIKE SHAPE, HAD BEEN DUG.

An abundance of broken arrowheads, as well as chipped or flaked pieces of rock in a definite locality, leads one to believe that this is a place where these objects were formed. The same is true of pottery and carved stone bowls and gouged pots. It is probably going too far to suggest that there was a Main Street with its various business sections, as we find in the large modern cities today, but if the segregation of various artifacts in definite places can be used as criteria, this comparison is not too far-fetched.

The camps and the things found there, about to be described, are located on the eastern slope of the Mule Mountains, Cochise County, Arizona, and

FIG. 7. HEARTH IN CENTER OF ROCK WALLS OF DWELLING

located from ten to fourteen miles from Bisbee (Fig. 1).

The first thing noticeable about the campsites is the abundance of pottery fragments. Many of them are undecorated; others show red painting on a buff background, and only occasional black on white, which came from the Mimbres, neighbors to the east. A very few small fragments of incised and coiled pottery have also been found. Trading with their neighbors must have gone on, or conquest, since sea-shell bracelets as well as different kinds of pottery other than that of their own manufacture are found in pure Red-on-Buff campsites.

Their pottery is painted on the inside, if it is dish-like, or on the outside, if it is bowl or olla-like, and in the latter case it is often beautifully polished on the inside to a smooth, black, satiny finish after having been smudged. Some bowls are painted both outside and in. It seems likely that the painted ware was their great pride and was used for show purposes and for food, either at meal times or for storage of grains.

There is also a great deal of pottery which is undecorated and unslipped which was used for water ollas and as storage for the various edible seeds (Fig. 2). Some of these were quite thick and heavy and were perhaps used for cooking because signs of fire, other than that due to the original firing, is seen. Much of the undecorated pottery is smudged, that is, blackened by carbon during the firing process and then polished.

Their main pottery was of four general shapes; a dish to eat out of, an olla to carry water, a bowl to store things in and a dish or scoop for transferring food from the dish to their mouths. The illustrations (Fig. 3) show the general types of the decorated tableware. After cremating their dead, they took the ashes and put them in one of the food bowls, placed an olla inside of this and the scoop over the top or to one side of the olla. These cremation sets, made especially for this purpose, were smaller than the regular pottery and were undecorated. This combination and its contents were then buried. The photos

FIG. 8. CIRCULAR OUTLINES OF FALLEN WALLS OF HOUSES OF RED-ON-BUFF CULTURE

show two sets of cremation bowls with the contents undisturbed. The ollas of largest size were most commonly of heavy unslipped pottery and unpainted. Practically all the ollas were rounded at the bottom and were probably suspended or partially buried, as they could not stand alone, while others had the Gila shoulder bottom which has a semi-rounded bottom and stands without support in a tilted position.

Pottery markers, circular in shape and an inch or more in diameter, are common, indicating the possibility that they played games. A handful of evenly matched almost round stones suggests marbles. In common with other primitive people they no doubt played many games.

The second thing which is noticeable is the abundance of work these people must have done besides the making of pottery, for there are rocks of all kinds and shapes which have been chipped, ground, whetted, flaked, bored or worn to a flat or curved surface.

There are metates or corn grinders, ground or gouged stone mortars, flat, long and narrow stone knives, stone combs, stone nose, ear and finger rings, arrow-heads of rhyolite, quartz, schist and greenstone, and chert, paint-mixing slabs and pots, both gouged and ground, chipping or mano stones, prayer rings, carved Gila monster, picks for digging, turquoise and stone beads, grinding stones for metates and for polishing pottery, boring stones for making holes in various beads, hammer stones, stone axes and stones that have been marked for purposes unknown (Fig. 3).

The metates are generally of granite and beside the small groove some have round cup-shaped depressions, indicating that they not only ground by rubbing but by using a long rock held vertically and rotating it on its long axis.

Of the stone implements, the combs (Fig. 3) are of greatest interest because they are not found elsewhere and because of the problem as to their use.

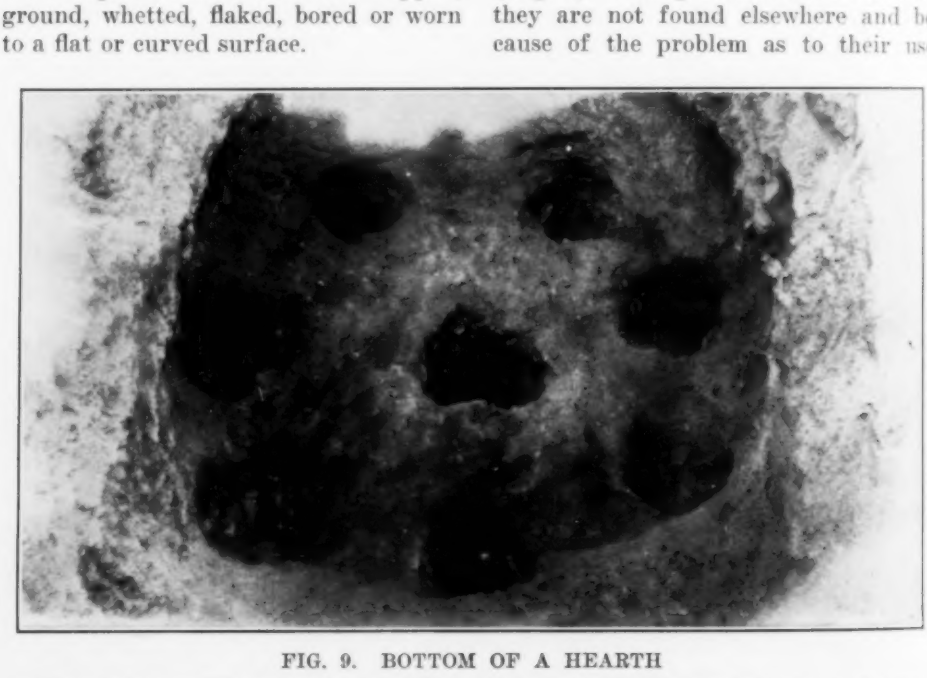


FIG. 9. BOTTOM OF A HEARTH

AFTER REMOVAL OF WALL TO SHOW THE SEVEN 18″ TO 2′ 0″ DEEP OUTER HOLES AND THE LARGER 2′ 0″ DEEP CENTER HOLE. NOTE: OUTER HOLES SLANT OUTWARD FROM CENTER AT ANGLE OF 70°.

FIG. 10. FLOOR OF DWELLING

SHOWING SHOULDER ON WHICH THE STONE WALLS WERE ERECTED.

They may have been used in combing the hair of humans, but they would have been of considerable use in combing the seed from cotton which they probably wove into clothing, for cotton grows native in Arizona. The combs are of red shaley sandstone, which could be flaked easily to a thin, wide shape and then smoothed into shape, or of a fine-grained, greenish colored sandstone or quartzite, which was formed into its shape with other rocks. In both kinds, the teeth were cut into the edge by using narrow, sharp-edged, harder rocks, as we use a file, and in all of them handles were shaped by chipping and grinding. They seem rather crude to us, but considering the material they had to make them out of and with, they did a very creditable piece of work.

The knives (Fig. 3) are of the same red, shaley sandstone and thinner than the combs. They were used for cutting meat, in skinning animals, preparing hides for tanning and many other purposes. Similar flat rocks without handles were used for scraping. The axes were used mostly for smashing and breaking purposes either on wood or stone, but one of them which is most beautifully symmetrical and is not a Red-on-Buff ax was probably used for ceremonial purposes. Except for the size of one of the axes, which weighed 19½ pounds, there is nothing unusual about them.

Bits of bright-colored stone, quartz crystals, unusually shaped stones and fossils are found in considerable number and lead to the belief that they were carried to camp for their unusualness, out of curiosity or as charms. But in the case of the quartz crystals it is noticed that all the points are worn off, indicating that they had been used for boring and carving.

Beads of turquoise of small size and having small holes, while not common and somewhat bleached by the sun, have been found. These stones probably came from the turquoise mines fifteen miles north of the campsites at Gleeson, Arizona. A small pendant of turquoise representing a frog had a hole bored in its head and was probably worn as a charm.

There are well-shaped, smoothly polished ear, nose and finger rings, made

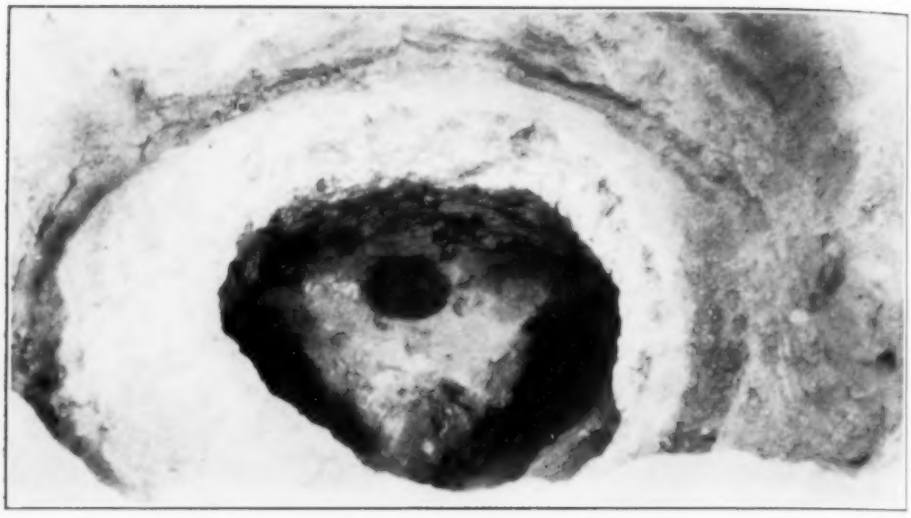


FIG. 11. A TYPE 7 HEARTH
SHOWING PARTIAL VIEW OF THE BOTTOM.

of a greenish shaley rock. The ear and nose rings were left with an opening, so that they could be worn without piercing the nose or ears.

The stone pots (Fig. 3) are smaller than those of pottery and were made either of sandstone or the softer more easily worked solidified lava mud from the southern foothills of the Chiricahua Mountains to the southeast. The latter are called gouged pots and were mostly shallow and very likely held paint which was used to decorate humans and pottery. They would be convenient also for lamps, the grinding of spices and for holding the fine clays used as slip on the pottery and many other purposes. From the illustrations (Fig. 3) it will be seen that they made most of these stone pots symmetrical in shape.

A stone pot, which shows their love for the unusual as well as their artistic ability, represents a lizard, Gila monster or some other kind of reptile and in its back is a depression similar to that found in their gouged pots (Fig. 3). The gouged pots were made by driving three or four slanting holes toward a common center and prying out the rock, leaving the desired depression, which was finally formed by chipping and rubbing down the rough surface, and since the solidified lava mud is quite soft, this was done easily with harder rocks. A prayer ring and various borders were made of a pumice-like lava. Prayer rings were used in addressing the water spirit of the earth.

Many of the manos, chipping stones and grinding rocks were made of fine-grained silicified sandstone, though schist, quartz, rhyolite, vesicular flow rock were used for metates, or milling stones, and had to be carried into their camps, since it is not found as float. Manos and broken or well-worn metates are very common. The well-used manos are rounded, but they are sharply angular when first made.

The arrow-heads show very fine workmanship, and many of their arrow-head-like borers are really works of art. Arrows were made by chipping or flaking with other harder rocks. Arrow-heads are not plentiful in the camps, but chips of all sorts with various kinds of edges

are common and were no doubt used for various purposes as knives, scrapers, borers, etc.

These people show a surprising stage of mental development, in spite of their living in the stone age, as is manifest by the number of conveniences employed. Their ingenuity is admirable, and they led happy, contented and useful lives.

Circular Pit-Hearth Houses

The most interesting features of these campsites are the hearths. In 1929, Mr. Mardon first discovered the camps and contented himself with looking for and finding artifacts. In the latter part of 1930, he found a segment of baked adobe projecting out of the ground and on digging found it to be part of what

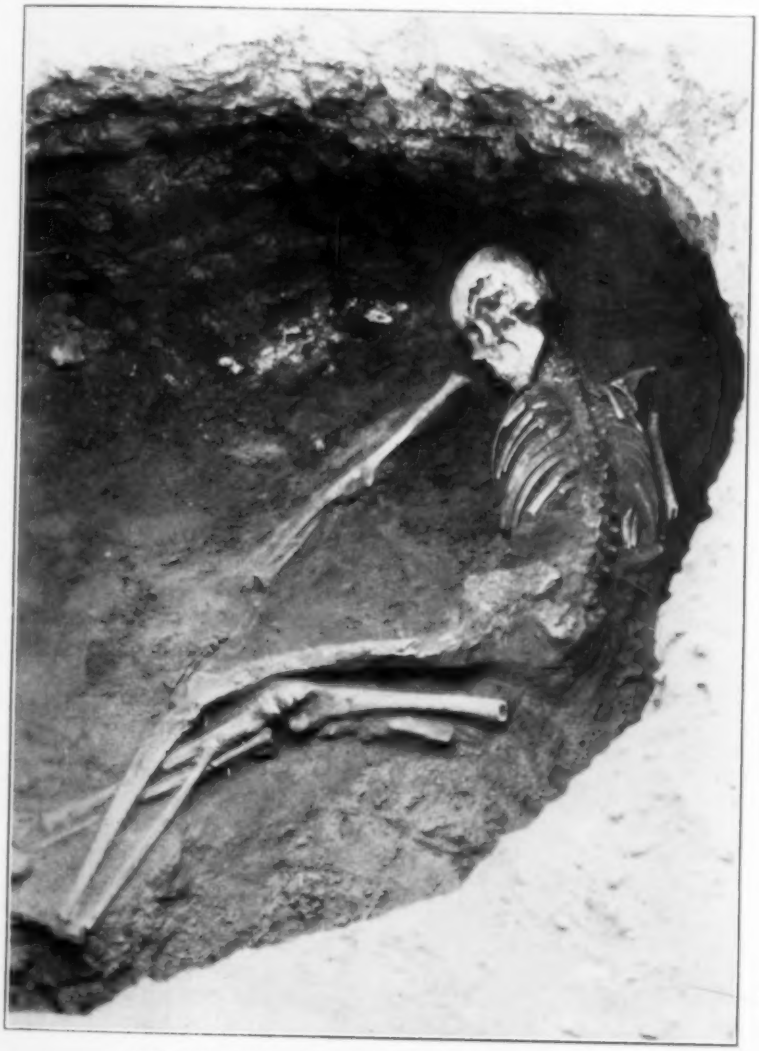


FIG. 12. SKELETON IN TYPE 7 HEARTH

From the position of this skeleton it would appear that the body was thrown in and covered with rocks.

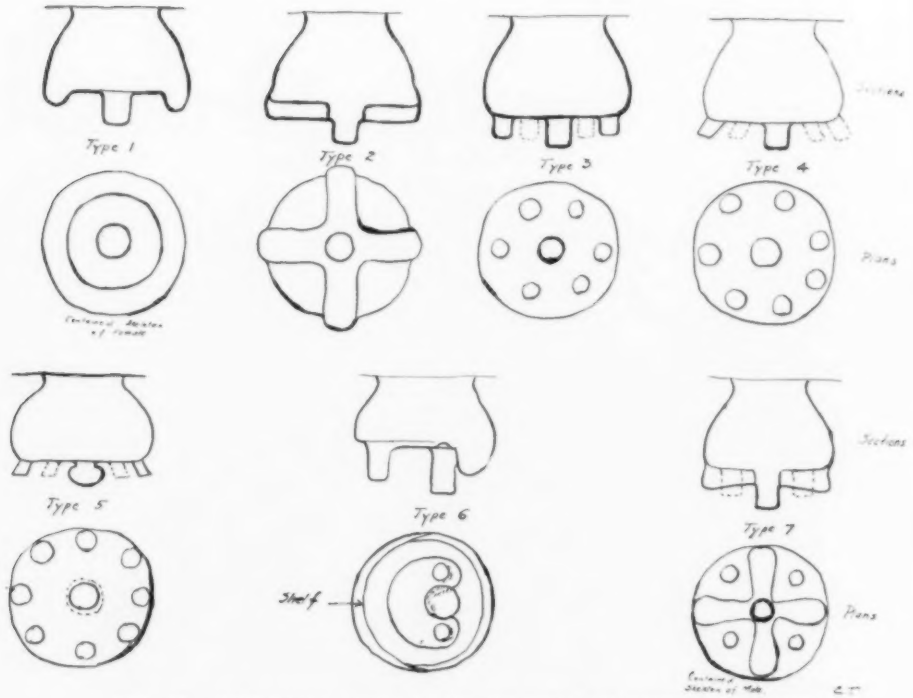


FIG. 13. SECTIONS AND PLANS OF RED-ON-BUFF HEARTHS IN CIRCULAR PIT HOUSES

appeared to be the grand-daddy of all ollas; but digging inside of it, there was disclosed a human female skeleton as shown in Fig. 4, Type No. 1. No, it was not an olla but a burial vault. That seemed certain; and then after the skeleton was removed, the hearth, Type No. 1, was disclosed, but even then it was not recognized for what it was, because the stones of the dwelling wall had been washed away (see Figs. 4 and 5).

In uncovering the hearth, Type No. 2 (Fig. 7), a segment of the stone wall was noticed, although it was originally found by the aid of the slightly projecting baked adobe of the hearth. No skeleton was found, but it had the eighteen to twenty-inch layer of stones on the hearth as did the first one and the three to four inches of baked adobe mud as walls.

The circular arrangement of stones had been noted in the camps before (page 423), but was considered as being the remains of ancient tanques or cisterns for catching and storing water. Digging in the center of these circles of stones disclosed the remaining hearths shown herewith. All the hearths are in the center of what were at one time circular stone dwellings, which were twenty to thirty feet in diameter, and they extended three to five feet below the level of the floor of the dwelling.

Only one other skeleton, the skull of which is shown, was found in the hearths, and thus far seven distinct patterns of hearth bottoms have been found. Photos and sketches of these are given herewith (Figs. 4–13, inclusive).

All of them are smaller at the top, three feet to four feet across and wider at the bottom and olla-rim shaped at the top. They suggest a large buried olla.

In making the hearth, a hole was dug, having the shape of the hearth, and then the walls were plastered with adobe mud. The lining is three to four inches thick and baked by the fires which were subsequently built in them.

The hearths are the only ones of their kind so far found in connection with the Red-on-Buff culture and appear to have been a part of the early development of the culture, because in a later phase of the development the hearths were more

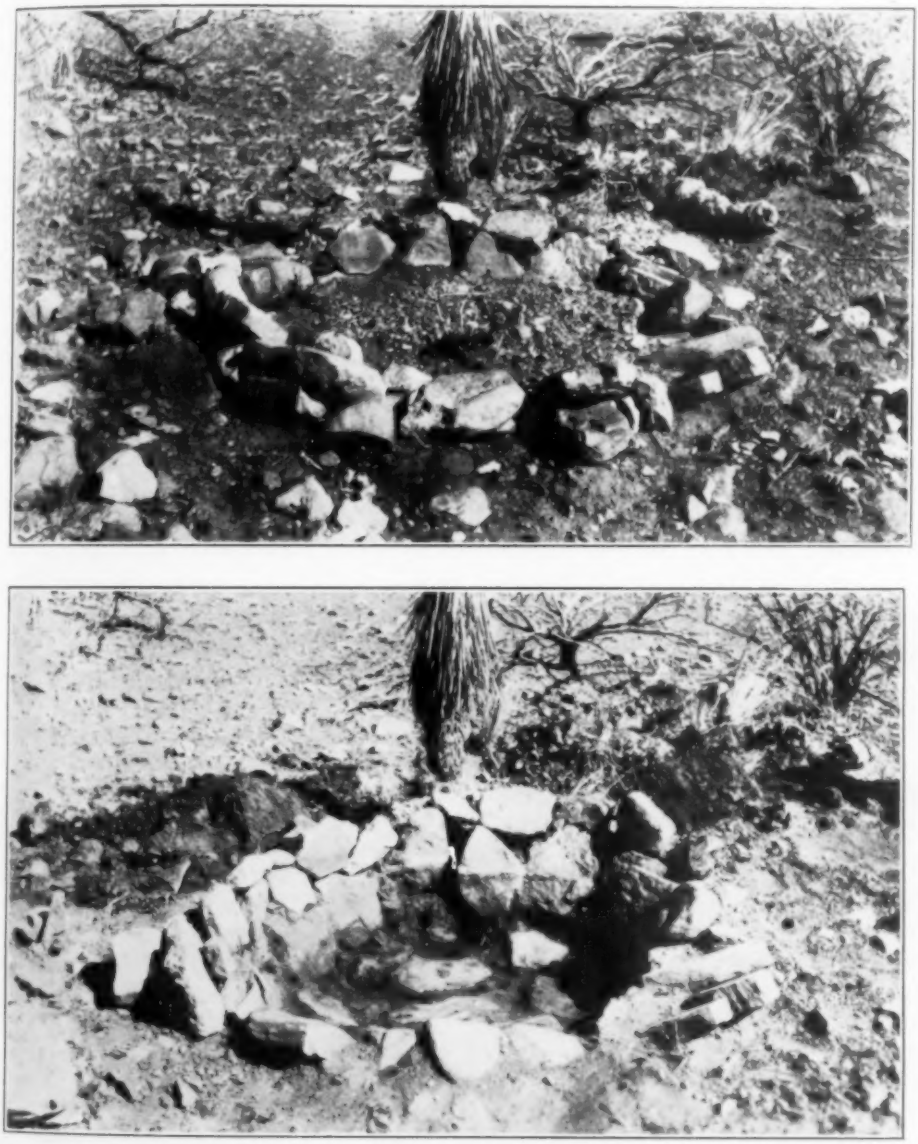


FIG. 14. SHALLOW HEARTHS OR FIREPLACES

Upper, AS FOUND; *lower*, AS EXCAVATED.

shallow and lacked the small pits in the bottom. Another sign of antiquity is the fact that but very little pottery was found in the hearths and most of it was undecorated.

The walls of the dwellings were built of stone two to four feet high and there was an opening or entrance on one side. The floor is four to six inches higher around the edge of the hearth than points at which the outer shelf begins, and the outer walls were built on this shelf. The shelf is two feet higher than the edge of the hearth and on the level of the general country. The houses were so-called pit houses and because of the hearth are given the name of "Circular Pit-Hearth Houses."

All the hearths contained ashes or charcoal and evidence of having contained numerous fires, but not necessarily large individual fires. There were also about eighteen inches of rocks on top of the hearth, in the interstices of which ashes and charcoal are found. It appears, from finding a hearth filled with rocks and no charcoal or ashes, that the kindling was laid, the rocks on top of this, and finally the fire was started. This heated the rocks, and these hot rocks were used to cook on and to heat the water for various purposes. This way there was a minimum of smoke. There are at least twenty or thirty more dwellings that are known but not excavated, and there were probably a great many more which have been destroyed by weathering. It is not possible to give a reasonable explanation of the holes in the bottom of the hearths, unless they were intended to catch the ashes for a number of fires. If they had been used as pottery kilns many fragments would have been found. An idea that occurs is that the bottoms may have had clan or religious significance. Of the ten hearths, so far found, only two hearth types or designs have recurred. The workmanship and general design lead to the belief that this type of hearth was in common use in the region and not original with the people of this camp, but a cultural trademark. At any rate, every dwelling could have sheltered a good-sized family, and the valley probably was reasonably populated at one time. The most likely purpose of the hearths was for cooking, roasting and heating purposes.

It is noticeable that a person, if stretched out with his feet at or near the edge of the hearth, finds his shoulder resting on the depression at the base of the shelf and his head on the shelf close to the house wall.

Many people could thus have occupied such a house, and with the floor covered with furs, it made a snug, safe shelter at night. The roof could have been made of inclined poles stuck in the ground outside of the stone wall and leaning on it, leaving a central opening at the top for the escape of smoke from the fire of the hearth. A covering of hides would not catch fire easily and would shed the rain.

It was not necessary to build a big fire to heat the stones. The tops of most of the hearths were found 1½ feet to 2 feet below wind-blown dirt which had been carried into the inside of the circular rock walls of the dwellings, filling the hearths as well, and the floors are well marked by fragments of burnt charcoal and occasional sherds of pottery. The floors are comparatively easy to find, because they are hard packed from use, while the wind-blown material is not packed.

The hearths were found by excavating in the center of the still remaining stone walls of the old dwellings and some of these rocks show signs of having been in a fire. No doubt the practise of using rocks of certain sizes on the fire was dictated by conditions and when a rock cracked due to heat and became too small it was thrown outside of the wall where it is now found.

Several stone-lined, circular, shallow

hearths, of which a photo of one is shown (Fig. 14) were found and one of these is in the center of a stone wall of a dwelling, from which it seems reasonable to suppose that both kinds of hearths were used. Were these smaller hearths the beginning of the larger hearths? These shallow open hearths must have been numerous, as ruins of them are common and many must have been destroyed because they were built so close to the surface. They give the impression of being summer hearths for cooking in milder weather, although the climate was perhaps semi-tropical at the time of occupation. The whole arrangement of the hearth and its surrounding walls was well calculated to keep the inside of the dwelling dry and warm in all seasons.

The skull, two views of which are shown (Fig. 15), when measured, gave a cephalic index of 84 which classifies it as a brachycephalic head or of the broad-headed type. It has been thought that the Red-on-Buff people were of the long-headed type, and they may have been, in which case the person to whom this skull belonged was an outsider, but this skull and another were found inside the large deep hearths and under conditions which lead to the belief that they were native Hohokam. More skulls must be found, however, to determine or decide this question. The fact that these people cremated their dead will make the question more difficult to ascertain.

Designs of Pottery

Copies of various designs found on pottery are shown (Fig. 16) and are considered to be clever in design and good in execution. When it is remem-

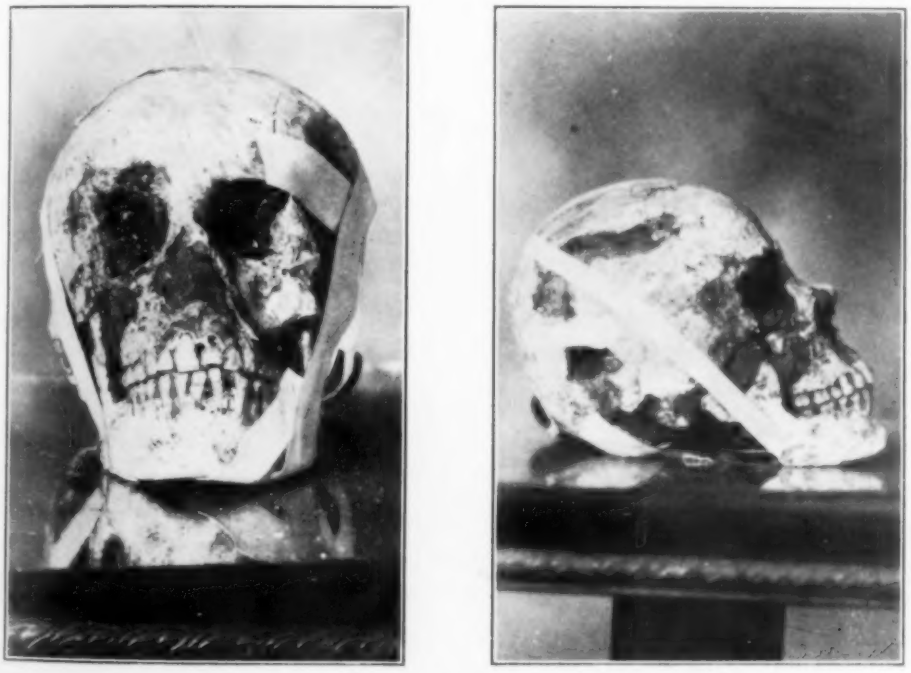


FIG. 15. SKULL OF SKELETON FOUND IN NO. 7 TYPE HEARTH IS BRACHYCEPHALIC

Cephalic Index 84.

EXAMPLES OF DESIGNS OF RED-ON-BUFF POTTERY, near Bisbee, Ariz.
DECORATION OF INSIDE AND SECTION OF BOWL
FAGMENT DECORATED OUTSIDE AND SECTION OF GILA SHOULDER OLLA
SHERDS SHOWING VARIOUS DESIGN MOTIVES.
DECORATION OF INSIDE AND SECTION OF BOWL
DECORATION OF INSIDE AND SECTION OF BOWL

FIG. 16. EXAMPLES OF DESIGNS OF RED-ON-BUFF POTTERY, NEAR BISBEE, ARIZONA

bered that no potter's wheel or other means of laying out or putting on the work was known to these people, their craftsmanship is to be admired. For brushes they used the maguey or century plant, by cutting pieces lengthwise of the fiber to the width desired and fraying the ends. Numerous pieces of hematite were found in the camps, indicating that it was used for paint, and in order that a sharp continuous line may be drawn, it must have been necessary

to mix this finely ground material with some plant juice and water.

The background of the painted or decorated pottery which is buff owes its smoothness to slipping. Slipping is the art of applying a thin layer of fine clay on the rough surface of the green or burned pottery. In the latter case a second firing is necessary. The design and condition of the pottery come in the classification of the early sedentary period of this culture, say approximately 200 A. D., although this date may be pushed back or ahead, depending on evidence which may be found later.

Irrigation

The culture has remarkable ditches to its credit in many localities and they are found to a limited but distinct degree in association with the campsites being discussed here. This phase of the campsites has been investigated only casually. They also built stone walls as terraces to hold the soil level and as a means of irrigating. A ten-acre field close to one of the campsites is studded here and there with irregularly placed heaps and cairns of rock which were piled up to permit farming and irrigation.

Although there is no running water, except during floods of rain, from the Mule Mountains into the valley at the present time, there probably was during the occupation of the campsites. Dry farming is a precarious means of existence under existing climatic conditions, and there are no springs, except in the mountains, so that for water these people must have had springs or running water closer to their sites than found at present.

Defense

There are no encircling walls or means for defense in the campsites. Perhaps none were needed. The campsites are on no rise of ground but just out in the open. Evidently there were no wild animals that were especially feared by the community or from which it needed protection, and human enemies were either few or the Hohokam sufficiently schooled in open warfare in case of a surprise attack to be able to defend themselves without ramparts or defense walls.

At a distance of three to four miles from the sites, however, on top of Abbott's Peak, there were found six or seven circular stone walls with entrance openings well preserved which were evidently used as shelters by lookouts of the Hohokam. A splendid view of an unusually wide range is had from this peak and signal fires either day or night could easily inform the community of the approach of friend or enemy.

Abbott's Peak is crowned by a cliff of limestone which encircles the top in a manner which permits entrance to the top from only one place about fifty yards wide. At this place are found the remains of two or three successive parallel stone walls, which at one time were higher than at present and were put there for defensive purposes.

At several places at the base of or slightly below the cliff, metates, grinding stones, sherds of pottery and signs of dwellings were found, indicating that this peak was at one time occupied by a group of people either on the defensive or merely as lookouts or guards. There may be other look-out points, but they are not known. Much has still to be learned about this remarkable culture, but the information so far gathered in this small area, it is hoped, will add something to the sum total of what is already known and help toward unravelling the interesting but complicated and incomplete picture of the Hohokam.

ANTHONY VAN LEEUWENHOEK AND HIS MICROSCOPES[1]

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WHAT the telescope is to the astronomer the microscope is to the biologist! Since the physical requirements of both these instruments are much the same, it is not surprising to find that the improvement and increased applications of one follow much the same path as do those of the other. Long before microscopes had been invented, however, simple lenses were in use. These lenses, doubtless employed for the inspection of small objects, were certainly known to the ancients. Pieces of rock crystal ground into biconvex forms were found by Layard in the ruins of Assyrian Nimrud, and Seneca tells us that hollow spheres of glass filled with water were used by the Romans to produce large images. The cutting of gems, an art which reached such high perfection in antiquity, must have been done under some kind of magnifier. In later times Roger Bacon (1214–1294) described a crystal lens as "useful to old men and to those whose sight was weakened, for by this means they will be able to see the letters sufficiently enlarged however small they may be." Spectacles were known to Chaucer (1340–1400), for he puts into the mouth of the wife of Bath the saying that poverty is a spectacle through which a man may see who are his real friends.

Yet notwithstanding these applications of lenses to the affairs of daily life, early science does not seem to have used such devices. The truly wonderful observation of Aristotle on the developing chick appear to have been made exclusively by his unaided eye, for he gives no intimation in his writings, so far as I am aware, that he ever used any artificial means as an aid to his vision. Pliny in his natural history has much to say about lentils but nothing about lenses.

The simple microscope, a biconvex glass mounted in a metal carrier, appears first to have come into general use at about the time of the Thirty-Years War (1618–1648). This instrument gained great popularity in that it was the means by which the movements of diminutive living creatures could be exhibited. Of these none was more frequently shown than the flea; hence the name of this device "vitrum pulicare" or, in common language, flea glass.

With the advent of the simple microscope there sprang up in Europe during the seventeenth century a flourishing school of workers, the early micrographers, who became enthusiastically engaged in the study of the minutiae of nature and who, by the use of the microscope, opened up a whole world of living forms never dreamed of by their predecessors. Chief among these early enthusiasts were Marcello Malpighi (1628–1694) of Italy, Robert Hooke (1635–1703) and Nehemiah Grew (1641–1712) of England, and last but not least Jan Swammerdam (1637–1680) and the subject of this essay, Anthony van Leeuwenhoek (1632–1723), both of Holland.

The lives of these two Dutchmen were almost exactly coincident with the golden age of the Netherlands. Following the struggles of William of Nassau the Dutch Republic gained its effective

[1] The author is under great obligation to Mr. Clifford Dobell for permission to quote freely from his recent volume, "Anthony van Leeuwenhoek and His Little Animals."

independence in 1609 and from that time onward it grew immensely in power and in riches. The middle of the seventeenth century may be taken as the culmination of this growth. At that time Amsterdam was the greatest trading center of Europe and the Dutch East India Company and the West India Company were the most powerful and successful commercial enterprises in the world. From a soil thus enormously enriched by commerce there arose an unprecedented artistic and intellectual efflorescence. Social life expanded, the arts flourished and science blossomed as never before. This was the time of the great Dutch painters Jan Vermeer, Jan Steen, Franz Hals and, above all, Rembrandt. At this time flourished the celebrated Dutch physicist, astronomer and mathematician, Christian Huygens, and the anatomists Franciscus Sylvius, Nicolaus Steno and especially Reinier de Graaf. Even the unassuming, excommunicated lens-grinder, Spinoza, brought to philosophy during this period a novel exaltation. Into this ferment of new life and ever-increasing activity were born the Dutch micrographers already named, Swammerdam and Leeuwenhoek. Although they were sons of one civilization their positions in life could hardly have been more in contrast.

Swammerdam, whose life has been completely sketched by Miall, was a native of Amsterdam and born into reasonable affluence. His father, a well-to-do apothecary, had established a private museum of natural history. Swammerdam the son was intended for the ministry, but he finally prevailed upon his family to let him study medicine. Meanwhile natural history became his passion and insects his special care. He attended the University of Leiden, where he made the acquaintance of Steno, de Graaf and Sylvius. He then traveled in France. On his return to Amsterdam he continued his anatomical and medical studies and perfected himself in methods of delicate dissection and injection under the simple microscope. In 1668 Cosmo, duke of Tuscany, visited Amsterdam, and when Swammerdam opened for him a fully grown caterpillar and showed him the wings, legs and other parts of the future butterfly in process of formation and packed away under the larval skin, Cosmo was so impressed that he offered Swammerdam a large sum of money for the latter's collections, provided that Swammerdam would come to Florence and settle there under the patronage of the duke. This offer Swammerdam finally declined, probably on religious grounds. In 1669, when he was thirty-two years old, he published his "General History of Insects." His health was far from robust, and disagreements with his father, who looked upon natural history as not a gainful occupation, led to difficulties. He was ambitious to complete further work on the insects and particularly on the day-flies and the hive-bee, and this he did despite a certain amount of parental opposition. His account of the day-flies was published in 1675. Five years later, in 1680, after serious illness and many financial worries, he died at the early age of forty-five. Thus Swammerdam's life reached its conclusion, the life of a man academically trained, reasonably traveled, cultured, dependent chiefly upon his family and short in years, in almost every respect the very opposite of that of his compatriot Leeuwenhoek.

Anthony van Leeuwenhoek was born at Delft in 1632. His life and doings have been most sympathetically and delightfully recorded in a recent memorial volume by Clifford Dobell, a scholar to whom all others are indebted for this labor of love. The parents of Leeuwenhoek were of good Dutch stock, his father a basket-maker of no personal or social distinction, his mother the

daughter of a well-to-do Delft brewer. Of their seven children Anthony was the fifth, born in the tenth year of their married life. The father died in 1638 when Anthony was five years old, and the mother, after a second marriage, lived till 1664. About the time of his mother's remarriage Anthony was sent to school at Warmond near Leiden. Apparently he never looked forward to a university career, for at the age of sixteen he went to Amsterdam, where he entered a linen-draper's shop to learn the business. While he was in Amsterdam it seems probable, according to Dobell, that he made the acquaintance of Swammerdam some five years his junior. After six years' apprenticeship in Amsterdam Leeuwenhoek returned to Delft, where he lived for the rest of his days. The Thirty Years War had ended, peace was reasonably assured for the time being, and Leeuwenhoek settled down into the quiet life of his native place.

Much of the quaintness and beauty of old Delft still remains. To the stranger of to-day, wandering along its quiet canals and shaded streets, it is easy to imagine scenes such as must have inspired the brush of an artist like Vermeer. In fact Vermeer's "View of Delft" and his "House on the Street," now both well-known classics in art, may be taken as evidence of how little the modern town has changed from that of the seventeenth century. It is a strange coincidence that on the death of Vermeer, which occurred at Delft in 1675, Leeuwenhoek was appointed official receiver to settle the greatly reduced estate of this noted artist. Life was even more circumscribed in those days than it is now.

In 1654, when Leeuwenhoek was twenty-two, he married. From this union were born five children, three sons and two daughters, only one of whom, Maria, lived beyond infancy. Leeuwenhoek's marriage year was notable in several respects. It was the year in which the great powder magazine in Delft exploded, killing hundreds of people and destroying many houses. This year likewise was the year in which peace was declared between England and Holland. It is not surprising then that at this time Leeuwenhoek should have bought in Delft a house and a shop and set himself up in business as a draper. According to bills now extant, he was buying and selling cloth, silk, ribbons, tape, buttons and the like. In 1666 his wife died, but in 1671 he married again, his second wife living till 1694. After her death his household was managed by his only surviving daughter, Maria, who is described by Dobell as devoted to her father. She cared for him in his old age when he was weak and infirm, and over his grave she raised a fitting memorial. She herself lived to be an aged woman, for she attained her eighty-ninth year. On her death her body was interred in her father's tomb.

Although Leeuwenhoek's business must have taken much of his time, he accepted in 1660 an appointment as Chamberlain of the Council-Chamber of the Worshipful Sheriffs of Delft. The duties implied in this appointment may be gathered from the wording by which it was ordered and which has been translated by Dobell as follows:

> Their Worships the Burgomasters and Magistrates of the Town of Delft have appointed and do hereby charge Antony Leeuwenhoek to look after the Chamber wherein the Chief Judge the Sheriffs and the Law Officers of this Town do assemble: to open and to shut the foresaid Chamber at both ordinary and extraordinary assemblies of the foresaid Gentlemen in such wise as shall be required and needful: item to show towards these Gentlemen all respect honour and reverence and diligently to perform and faithfully to execute all charges which may be laid upon him and to keep to himself whatever he may overhear in the Chamber: to clean the foresaid Chamber properly and to keep it neat and tidy: to lay the fire at such times as it may be required and at his

own convenience and carefully to preserve for his own profit what coals may remain unconsumed and see to it that no mischance befall thereby nor from the light of the candles: and he shall furthermore do all that is required of and that pertaineth to a good and trusty Chamberlain. For the which service the foresaid Antony Leeuwenhoek shall enjoy such wages benefits and emoluments as the foresaid lamented Jan Strick his predecessor in office did enjoy and shall enter into his duties upon the morning of the 24th of January 1660 and his wages shall be paid upon the same terms as those whereon the foresaid Jan Strick's were paid. Ordered by the Burgomasters in Council assembled this 26th day of March 1660 and signed by

J. CAMERLING. *Pensionary.*

In return for the performance of the duties as Chamberlain, Leeuwenhoek received 314 florins a year, a sum later increased to 400. Subsequently he added to this position that of "Generaalwijkmeester" or alderman, for which he received an additional annual stipend of 50 florins. Both these appointments, according to Dobell, were probably sinecures and their duties were performed by proxy. In 1669 Leeuwenhoek was admitted as a surveyor within the jurisdiction of the Court of Holland, a step which he could not have taken, had he not had considerable knowledge of applied mathematics. He was now in his thirty-seventh year, a well-established and respected tradesman in Delft.

Thus far Leeuwenhoek had had no outward connections with the learned world, but in 1673 he began those associations with the Royal Society of London that were to extend throughout half a century and were to be terminated only by his death. How these associations arose has been fully narrated by Dobell. The Royal Society of London, founded about 1660 for the promotion of natural knowledge, was desirous of getting into communication with all those who sympathized with its aims. To this end its first secretary, Henry Oldenburg, carried on a voluminous correspondence with savants the world over, and among these was Reinier de Graaf, the friend and townsman of Leeuwenhoek. Through Oldenburg, de Graaf learned of the new and remarkable microscope made by Eustachio Divini in Italy, by which the inventor had been able to discover "an animal lesser than any of those seen hitherto." That Holland should not be outdone by Italy de Graaf addressed the following words to Oldenburg as translated by Dobell:

That it may be the more evident to you that the humanities and science are not yet banished from among us by the clash of arms, I am writing to tell you that a certain most ingenious person here, named Leewenhoeck, has devised microscopes which far surpass those which we have hitherto seen, manufactured by Eustachio Divini and others. The enclosed letter from him, wherein he describes certain things which he has observed more accurately than previous authors, will afford you a sample of his work: and if it please you, and you would test the skill of this most diligent man and give him encouragement, then pray send him a letter containing your suggestions, and proposing to him more difficult problems of the same kind.

De Graaf's letter was accompanied by a sample of Leeuwenhoek's work, which included observations on mould, the sting, mouth-parts and eye of the bee, and the louse. These observations so interested the members of the Royal Society that its secretary was instructed to communicate with Leeuwenhoek, who replied as follows:

I have oft-times been besought, by divers gentlemen, to set down on paper what I have beheld through my newly invented *Microscopia:* but I have generally declined; first, because I have no style, or pen, wherewith to express my thoughts properly; secondly, because I have not been brought up to languages or arts, but only to business; and in the third place, because I do not gladly suffer contradiction or censure from others. This resolve of mine, however, I have now set aside, at the intreaty of Dr. Rag. de Graaf; and I gave him a memoir on what I have noticed about mould, the sting and sundry little limbs of the bee, and also about the sting of the louse. This memoir he (Mr. de Graaf) conveyed to you; whereupon you sent me back

an answer, from which I see that my observations did not displease the Royal Society, and that the Fellows desired to see figures of the sting and the little limbs of the bee, whereof I made mention. As I can't draw, I have got them drawn for me, but the proportions have not come out as well as I had hoped to see 'em; and each figure that I send you herewith was seen and drawn through a different magnifying-glass. I beg you, therefore, and those Gentlemen to whose notice these may come, please to bear in mind that my observations and thoughts are the outcome of my own unaided impulse and curiosity alone; for, besides myself, in our town there be no philosophers who practise this art; so pray take not amiss my poor pen, and the liberty I here take in setting down my random notions.

According to Dobell, a week before Leeuwenhoek sent to the Royal Society the letter just quoted Constantijn Huygens, diplomat and father of the physicist, wrote as follows to Hooke:

Our honest citizen, Mr. Leewenhoeck—or Leawenhook, according to your orthographie—having desired me to peruse what he hath set down of his observations about the sting of a bee, at the requisition of Mr. Oldenburg, and by order, as I suppose, of your noble Royal Society, I could not forbear by this occasion to give you this character of the man, that he is a person unlearned both in sciences and languages, but of his own nature exceedingly curious and industrious, as you shall perceive not onely by what he giveth you about the bee, but also by his cleere observations about the wonderfull and transparent tubuli appearing in all kind of wood. . . . His way for this is to make a very small incision in the edge of a box, and then tearing of it a little slice or film, as I think you call it, the thinner the better, and getting it upon the neddle of his little microscope—a machinula of his owne contriving and workmanship—brass. . . . I trust you will not be unpleased with the confirmation of so diligent a searcher as this man is, though allways modestly submitting his experiences and conceits about them to the censure and correction of the learned. . . .

Thus through these several agencies was Leeuwenhoek introduced to the Royal Society and thus began that remarkable series of communications in which he contributed to the society a fund of knowledge made possible by the use of his microscopes.

It seems quite clear that for some years previous to 1673, when Leeuwenhoek became known to the Royal Society, he had been grinding lenses and making microscopes, but when and how these activities began no one appears to know. Suffice it to say that in the year mentioned he had gained no small degree of skill in this practise and that this skill and its results were recognized by such men as de Graaf and the elder Huygens.

The instruments commonly designated as Leeuwenhoek's microscopes are fortunately fairly well known to us. At his death, as can be ascertained from the auction list of certain of his effects, it is known that he left 247 completely finished microscopes and 172 mounted lenses, or 419 pieces in all. The great majority of these have disappeared, including the "little cabinet, lacquered black and gilded that comprehendeth within five little drawers wherein lie inclosed 13 long and square little tin cases which I have covered over with black leather, and in each of these little cases lie two ground magnifying glasses making 26 in all everyone of them ground by myself and mounted in silver." This cabinet was bequeathed by Leeuwenhoek to the Royal Society, safely received by it, not infrequently inspected by its Fellows, but finally duly and totally lost. In fact Dobell reports that of the 419 lenses known to exist at the time of Leeuwenhoek's death, at present only eight can be accounted for with certainty and all these are held on the continent, England and America possessing none. These eight, however, with the descriptions of others, give us ample ground for reconstructing the instrument.

This consisted of two parallel plates of metal, brass, silver or even gold, held together by rivets and carrying between them in their upper central part one of the precious lenses held firmly opposite holes in the plates, so that the observer could put his eye next the united plates

and peep through the aperture with its contained lens at any object held on the opposite side. Such object was not supported by the hand but by a metal point which terminated a rod itself attached to the lower part of the plate in such a way that it could be moved by threaded screws into appropriate position for inspection and clear focus. The object to be examined was either mounted on the metal point by being stuck there with glue or it was placed on a bit of thin glass or mica or between two pieces of such material or in a capillary tube. These partial mounts were attached of course to the metal point. Thus a great variety of substances could be brought under inspection. Leeuwenhoek's method of demonstration was such that he usually made a microscope for each object and having set up the whole, he retained it in that form for inspection instead of changing the object under the glass, as was later commonly practised.

It will be seen at once from this brief description that Leeuwenhoek's so-called microscopes were little more than simple lenses and that they bore no resemblance whatever to the compound microscope of even his day. This instrument, in which one lens or lens-system magnifies the image formed by another, was indeed known at his time, but it was so imperfectly developed that even the relatively crude magnifier made by Leeuwenhoek far outran it in efficiency. The great success that Leeuwenhoek's microscopes attained turned, it appears, on the marvelous skill with which he ground his lenses. In early times, when many of these lenses were available, they were carefully examined by expert opticians and were pronounced by them as unusually free from optical defects, notwithstanding that they were of relatively high power. His better lenses are known to have magnified 270 diameters, and it is possible that some of his best products, of which he seems to have been very choice, reached as much as 300 diameters.

A perusal of Leeuwenhoek's letters and the comments of his contemporaries show that he guarded his best work with a certain jealousy and secrecy. He often implies in the accounts of his observations that he could see what many others could not. He intimates further that he possessed methods of manipulation which placed him personally at an advantage over others and which he was loath to impart to them. This professional secrecy, if such it was, formed a part of his psychology and Dobell has rightly called attention to it. What it was all about is not easy to surmise. It is, however, certain that he observed and figured many bodies that it would be very difficult for even an unusually skilful observer to make out with the equipment ordinarily attributed to Leeuwenhoek. That he had certain remarkable lenses at his disposal that never were allowed to come to light is very unlikely. His secret, if he had one, was probably in his method of manipulation. Dobell has suggested that he may have used some form of indirect illumination, a step entirely possible with his apparatus. When it is recalled how greatly this method would have improved the possibilities of viewing very minute objects, such as bacteria and the like, it seems more than probable that Dobell is correct in his surmise and that this was the step in Leeuwenhoek's manipulation that set him so far in advance of his competitors and that he was so loath to reveal. In support of this suggestion Dobell quotes Leeuwenhoek, who in reply to a criticism remarks, "but I can demonstrate to myself the globules (corpuscles) in the blood as sharp and clean as one can distinguish with one's eyes, without any help of glasses, sandgrains that one might bestrew upon a piece of black taffety silk." Precisely this comparison would hardly have occurred to Leeuwenhoek had he not been accustomed to view microscopic objects on a dark background, such as is produced by in-

direct illumination. His vision was known to have been remarkably acute, but added to this there must have been some other turn in manipulation which gave him supremacy, and indirect illumination may have been that turn.

Equipped with unusual apparatus and technique, filled with a burning curiosity about all natural objects, and with fifty years of life ahead, Leeuwenhoek at the age of forty-one began a scientific career of remarkable discovery and accomplishment. His physical needs were met by the income from his shop and his town appointments. His time was largely his own and he was thus free to follow the passion of his later life.

During these fifty years Leeuwenhoek spread his scientific discoveries by writing letters mostly to the Royal Society, and many of these were published in its *Transactions*. In 1680 he was elected a fellow of the society, an honor which gave him great satisfaction. As a result of his work he became more and more widely known and was visited by celebrities as well as by others from all over the learned world. As Dobell writes, "kings and princes, philosophers and physicians and men of science, statesmen and clergymen and even common men went to see him and to look through his wonderful glasses." The visit of Peter the Great of Russia has been recorded with much detail. This monarch was so interested in what Leeuwenhoek had to show him that he detained Leeuwenhoek no less than two hours and on taking his leave shook Leeuwenhoek by the hand and assured the micrographer of his special gratitude. But Leeuwenhoek was not to be lionized. His heart was in his work and visitors took his time. Finally many were denied admittance, but he always endeavored to make himself accessible to those who came from the Royal Society. In this respect the following comment concerning visitors from Sir Hans Sloane, then secretary of the society, is not without interest. "Mr. Hans Sloane recommended Mr. Stuart to me, in his letter, as a curious Gentleman who has travelled through many countries; and the same Gentleman had two other Scottish gentlemen in his company, all of whom I gladly received, and so will I do all those who have an introduction from Mr. Sloane." Mr. James Petiver, however, was less fortunate. After an unsuccessful attempt to visit the micrographer in 1711, Petiver received from Leeuwenhoek a letter, from which the following significant extract is taken:

> I have received your Letter of the 2nd of August anno 1711, wherein you are displeased at not being welcomed at my house. I beg you please not take it ill, seeing that we send off everyone who tries to visit me unless they have some sort of introduction. . . . If you had kept by you the letter from Mr. Hans Sloane, you would not have missed a friendly entertainment at my house. And you were sent away especially because you were not known, and because some 8 or 10 days earlier no less than 26 people came to see me within four days, all of them with introductions (except a Duke and a Count with their Tutor): which made me so tired, that I broke out in a sweat all over.

Thus was poor Leeuwenhoek in his seventy-ninth year pestered by an importunate public.

Yet notwithstanding interruptions work went forward. In 1717 Leeuwenhoek really thought his end was near, and he wrote what he believed to be his last letter to the Royal Society, concluding with

> Methinks these will be the last observations I shall be able to send to you honourable Gentleman; because my hands grow weak, and suffer from a little shakiness, which is due to my advanced years, a good 85 having passed by me. And so I send you with this my deep thankfulness, because in the year 1679 you were so kind as to elect me, quite beyond my competence, a Fellow of the most worthy College of the Royal Society; and to send me a Diploma, together with two letters from the Secretaries of the Society, which likewise made known to me my election, by all the votes of the Fellows, then gathered together at a very full meeting.

I thank you also for the *Philosophical Transactions*, which Your Excellencies have been so good as to send me from time to time.

For all these honours and gifts aforesaid, I herewith convey to you my gratitude once more.

Yet after this letter Leeuwenhoek lived nearly six more years and sent to the society in that time no fewer than eighteen more epistles. But the end finally did come on the twenty-sixth of August, 1723. The Reverend Mr. Peter Gribius notified the fellows of the Royal Society of Leeuwenhoek's death, and his daughter, Maria, arranged his funeral and carried out his last wishes. Leeuwenhoek was buried in the Old Church of Delft on Sunday the thirty-first of August, 1723, "with 16 pallbearers and with coaches and tollings of the bell at 3 intervals." In 1739 his daughter Maria erected a monument to his memory in another part of the church where he now lies. Here she too was later buried. Thus concluded the life of a man who without a vestige of university training and utterly devoid of academic associations contributed to the scientific advance of his day as did scarcely another.

What these advances were may be mentioned only briefly. Leeuwenhoek confirmed and extended Malpighi's observation that the arteries connected with the veins by a system of very fine vessels, the capillaries. These Leeuwenhoek demonstrated in the tail of the tadpole and the eel, in the web of the frog's foot, and in the membrane of the bat's wing. He pointed out significant differences in the blood corpuscles of various animals. He was first to note the cross-striation of muscle fiber and the fibrous structure of the lens of the eye. Following the initial observation of Hamm, he began a study of spermatozoa, of which he believed he could distinguish two kinds, one giving rise to male offspring and the other to female, an anticipation of the modern doctrine but on wholly erroneous grounds. He studied cilia in the edible mussel and the embryos of the fresh-water clam and was a vigorous opponent of spontaneous generation. He studied the life history of the plant-louse and discovered viviparity. He watched fleas hatch from their eggs and showed that the so-called eggs of ants were their pupae. He plunged into the question of the nature of cochineal, and finally declared in favor of the view that it is a dried insect. He showed that the compound eye as such occurred in crustaceans as well as in insects. He studied the structure and breeding habits of spiders and watched them hatch from their eggs. He discovered rotifers and showed that they could undergo desiccation without consequent death. In the dried state he believed them carried from place to place as dust in the wind. He first described the fresh-water hydra, volvox, and so many of the flagellate and ciliate infusorians that he is usually regarded as the father of protozoology. He described the yeast plant and was the first to observe bacteria. These he studied and described from his own mouth, where he found them in and about his teeth. Such materials and a host of others claimed Leeuwenhoek's ingenious attention and afforded subject-matter for his eager, inquiring mind. Neither trained nor informed in speculative science he at times laid himself open to violent attacks from his opponents but, skilled beyond all others in direct observation, he amassed and handed down a body of knowledge prodigious in amount and unbelievable in importance. A simple, earnest, inquiring soul, in outward form an unassuming tradesman, in inward truth a burning flame of purest science.

A CENTURY OF PROGRESS IN THE CHEMISTRY OF NUTRITION[1]

By Professor H. C. SHERMAN

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THE occasion for which this paper is invited falls so near the completion of the first third of the twentieth century that we may conveniently divide our brief sketch of the progress of the chemistry of nutrition during the past hundred years into three periods of a third of a century each.

I

At the opening of the middle third of the nineteenth century, chemistry had accumulated a number of discoveries which we now recognize as fundamental to the science of nutrition; but little of what we could now consider an actual science of nutrition yet existed.

Lavoisier had shown the fundamental resemblance of oxidation of organic foodstuffs in the body with their burning in the air; Wohler had shown the possibility of the laboratory synthesis of substances previously known only as products of life; and several investigators had demonstrated the existence of organic catalysts of the sort that we now call enzymes; but these great discoveries had not yet been made effective through the building of the chemistry of nutrition as a clearly conceived and coordinated science.

This, Liebig was just about beginning to do a century ago. His first book on the significance of chemistry for agriculture and physiology appeared in 1840 and his classical treatise on animal chemistry followed only two years later. His principle, frequently called the "Law of the Minimum," was enunciated in 1843 and has ever since had an enormous influence upon the methods and interpretation of research in the nutrition both of plants and of animals.

Voit carried on the development of the chemistry of nutrition which Liebig had so effectively launched; for Voit, like his distinguished student, our dear friend the late Dr. Lusk, while bearing the title of professor of physiology, worked essentially in the chemistry of nutrition.

At the end of this middle third of the nineteenth century, the concept of chemistry as functioning in the nutritional processes in plant and animal life—in both the formation and the utilization of our foodstuffs—was being made effective for animal physiology by Voit at Munich, while in this country work much like Liebig's own, in its comprehensive grasp of the interrelated problems of the farm crop, the domesticated animal and man, was being done by Samuel W. Johnson, under whom Atwater was a graduate student at Yale in the late sixties, and under whom also Osborne began and developed his far-reaching researches upon the proteins.

Perhaps this is the point at which to explain that as time would not in any case permit the mention here of more than a fraction of those to whom honorable mention is due, no living person will be named in this review.

II

During the last third of the nineteenth century, the work of Pasteur was becoming fully effective.

[1] A paper prepared upon invitation of the American Chemical Society for its meeting at Chicago, September, 1933.

While the name of Pasteur is more intimately identified with fermentation and infection than with nutrition in the ordinary sense, yet Pasteur, in addition to being the chemist who became the father of modern bacteriology, was also the one who, perhaps more than any other, made clear and convincing the far-reaching principle that life processes are amenable to successful investigation by chemical methods and from the chemical point of view. In fact it has been said that the reason Pasteur progressed more rapidly than other bacteriologists in the study of disease was because he adhered to the chemical viewpoint and concentrated his attention not so much upon the morphology of the microorganisms as upon their relation to the substances which nourish them and the substances which they in turn produce.

Under such influences, then, as those of Pasteur, of Liebig and Voit, of Johnson and Atwater and Osborne, the latter part of the nineteenth century was both laying the scientific foundations for the science of nutrition and preparing the ground for its acceptance and for its effective service to human welfare.

Before the nineteenth century was over, the work of Osborne and others had added to chemistry a substantial list of proteins known as purified individual substances and ready to be studied with reference to the nutritional significance not only of their general chemical nature but also of their differences in chemical structure, particularly in their amino acid make-up.

Also, largely through the work of Frankland in England, of Thompson and Stohman in Germany and of Berthelot in France, the heats of combustion of the chief organic nutrients and of many of their metabolites had been determined quantitatively; Rubner had successfully studied the energy exchange of the body as a whole by direct experiments upon dogs, and Atwater by analogous and even more accurate experimentation upon men, so that the study and teaching of the energy relations of nutrition were beginning to take on the character of the exact sciences.

Many quantitative experiments had also been recorded in connection with the study of the protein factor in nutrition; and Osborne had published his finding (the significance of which was not fully appreciated until much later) of the protein nature of malt amylase—an enzyme typical of one group of the catalysts through whose action the energy value of the organic foods is brought with sufficient promptness into the nutritional service of the body.

Forster and a few others had given some attention to the rôle of the mineral elements in nutrition but appreciation of the importance of these was inhibited by preconceptions mistakenly carried over, without due discrimination, from the study of the nitrogen metabolism.

Eijkman had recorded what we now know to be good experimental evidence of the existence of a nutritional deficiency disease; but had, as a committee of the British Medical Research Council afterward phrased it, presented his findings "with so strong a pharmacological bias" as to obscure their nutritional significance, so that his work did not actually become a contribution to the chemistry of nutrition until it could be re-viewed in the light of the more chemical work which followed after the turn of the century.

From the American view-point, it is of interest to note that before the end of the nineties there was a strong center of advanced teaching and research in physiological chemistry at Yale, food chemistry was well developed in several of our colleges and universities, every state had in its Agricultural Experiment Station a center of research in agricultural chemistry which in many cases was

beginning to include consideration of the nutrition both of plants and of farm animals, and the United States Department of Agriculture was conducting research in the nutrition of man under the direction of Professor Atwater.

III

Gratefully acknowledging that we have reaped where our predecessors of the nineteenth century had sown, we may well emphasize the striking extent to which the present-day chemistry of nutrition has developed during our own first third of the twentieth century. The development of this newer knowledge of nutrition places upon the present generation of chemists a much heavier responsibility than has been borne by the chemists of the past, and opens to them a correspondingly greater opportunity for scientific scholarship and for service to human welfare.

Both in the interest of clearness and on logical and chronological grounds, we may speak of six steps in this line of scientific development, or (to change the figure) six "pillar concepts" on and around which our scientific structure is being built:

(1) The quantitative studies of the energy relations, now recognized as fundamental to the development of nutrition as an exact science; and now so far successful that one may speak with confidence and with precision of the chief substances whose oxidation in the body yields energy for its nutritional functions, the amounts of energy which these substances thus yield and the approximate amounts of energy which the body normally transforms at different ages and under different conditions. Current research in this field is constantly adding precision to our knowledge of the energy needs of normal nutrition and, in the hands of many workers, is showing more definitely what departures from the normal are to be expected in the different types and degrees of disease. The world-wide study of racial factors and climatic influences is also a far-reaching development in this field of research.

A connecting link with the second aspect of our subject is encountered when we ask how the chemical reactions upon which the energy exchanges depend are made to run fast enough in the body to meet its nutritional needs. A few words from the more familiar view-point of the proteins in nutrition must, however, intervene.

(2) Quickly following upon the markedly successful quantitative developments in the studies of energy relations, which were the outstanding news in nutrition at the turn of the century, came a truly epoch-making period of experimental enlightenment in the protein chemistry of nutrition. The proteins which Osborne, in this country, Fischer, in Germany, and others, had so systematically studied as chemical entities were now fed individually under closely controlled conditions to well-standardized laboratory animals and the chemical structures and nutritional relationships were correlated in terms of the qualitative and quantitative differences of the individual amino acids which the food proteins supply.

And here a most interesting and important two-fold development has occurred. On the one hand it has been found that some of these amino acids are more or less interchangeable while some are individually quite indispensable, each having evidently its own unique function or functions in nutrition. And on the other hand it has been found that several of the catalysts directly or indirectly essential to the energy metabolism, for instance, the glutathione which appears directly to catalyze the oxidation process, thyroxine and adrenaline which in some way expedite the process, insulin which facilitates some intermedi-

ate or preparatory reaction, and the typical hydrolytic enzymes which bring the organic foodstuffs into forms upon which these other catalysts can act—all these now appear to be derivatives of amino acids such as result from the digestion of food proteins.

Such discoveries point the way to still further scientific advances and they have an enormous practical value in showing how the proteins (or protein mixtures) of different articles of food can supplement each other with most efficiency and economy both in human nutrition and in the feeding of farm animals.

Tempting as it is to dwell longer here, we must turn now to the third of the six steps or aspects in which we are seeking to indicate (how be it all too meagerly!) the recent development of our subject.

(3) Gradually, during the past thirty years, in part following upon the twentieth century reform in our ideas of the protein factor in nutrition and in part independently, there has come an awakening to the importance of several of the mineral elements. The fact that several of these elements are regularly found in the body, and the effects which had been observed in a few rather primitive experiments with diets in which they had been very drastically reduced, had been generally accepted as indicating that they play some part in nutrition, but the amounts needed were usually underestimated and it was generally assumed that chance intakes would practically always be adequate. The investigations of the past thirty years have dispelled this fallacy with regard to one mineral element after another and shown that it is both scientifically significant and practically profitable to give these elements the same careful attention that we give to protein. The practical importance of the mineral elements as factors in food values is even greater in human nutrition than in animal feeding, because of the tendency to over-refine human foods and to divert to the feeding of animals those "coarser" parts which are usually richer in the mineral elements.

Recent advances in our knowledge and appreciation of the rôle of the mineral elements in nutrition has given us the key, certainly to the prevention of much of the malnutrition hitherto so prevalent, and we hope also to a constructive advance in our standards of positive health.

(4) Almost simultaneously with this gradually dawning realization of the importance of the mineral elements, and doubtless obscuring it in the view of many, there has come a dramatic series of discoveries regarding the existence in food and the significance in nutrition of a whole group of substances, not particularly related in their chemical natures or nutritional functions, but which are commonly called the vitamins.

Scientifically the vitamins should not go under a group name for they are not a sufficiently similar or related group of substances. The more we learn about the vitamins the less alike do they appear. That they have been so grouped is the accidental result of their existence having been discovered and their nutritional importance established in too rapid a succession for the actual physical isolation and complete chemical identification of the substances to keep pace, even though many able chemists have been working actively upon the pure chemistry of these compounds.

When this situation is fully grasped it constitutes in itself a most impressive demonstration of the rapidity with which so many fundamentally important discoveries have been made in this field in the past few years.

What from habit is still called the vitamin concept is in reality a whole group of concepts, each unique.

Even so, there are still other concepts which we should also consider.

(5) Another important aspect of the recent progress in the chemistry of nutrition is found in the interrelationships between the different nutritional factors. Thus, as already noted, the energy metabolism depends upon catalysts containing or derived from the proteins or their amino acids. Or, again, in hemoglobin, the body makes itself a substance which is outstanding both as a body protein and as an iron compound and which plays a leading rôle both in transporting oxygen for the energy metabolism and in maintaining the acid-base balance.

Still again, research upon the interrelationships of calcium, phosphorus, and vitamins A, C and D has added greatly to our understanding of the chemistry of the body and has shown the way to the conquest of rickets and scurvy—and, we hope, to the building of better teeth.

We also have reason to hope that the chemistry of nutrition is well on the way toward a solution of the pellagra problem through the working out of the nutritional interrelations between vitamin G and whatever other factors are involved.

If something of the order of thirty-odd chemical entities (elements or compounds as the individual case may be) must be nutritionally supplied to the body from without,[2] besides all the substances that it makes within itself, application of the algebra of combinations and permutations will soon show that a human life-time would not be sufficient for the consideration of all possible interrelations. Plainly this promises to be almost too fertile a field, a region in which the investigator must guard against losing himself in a veritable jungle growth of interrelationships.

As a compass or polar star for our guidance, however, we have what we are here calling the sixth step or aspect in our present-day concept of the chemistry of nutrition.

(6) This is the principle that in the chemistry of nutrition our ultimate concern is essentially the nutritional reactions of the living body as a whole. For the living body is a coordinated whole: and such a whole is something more than a mere summation of its parts. However interesting and important it may be to follow individual chemical substances into individual bodily organs and to trace detailed interrelations among the many factors essentially involved in nutrition—and it is thus that much of our knowledge has been, and must be, won—we have recently learned the importance of remembering also that, somewhat as in practical affairs, the proof of the pudding is in the eating, so in the most far-reaching ambitions of research in nutrition, the court of last resort is the experiment, or closely-reasoned series of experiments, conducted with all possible exactness and the most careful laboratory control, upon the whole animal, often throughout its whole life-cycle, and sometimes even through successive generations.

The demands of brevity confine us here to a mere hint at the far-reaching

[2] At the time of writing (September, 1933) the views of different chemists as to what the body needs nutritionally from without would range from twenty-five items upward, as indicated by the following summary: (1) At least fifteen of the chemical elements in the strict chemical significance of that term—carbon, hydrogen, oxygen, nitrogen, calcium, phosphorus, potassium, sulfur, sodium, chlorine, magnesium, iron, iodine, copper, manganese, and perhaps others; (2) from four to fourteen of the amino acids furnished by the proteins of the food; (3) probably at least a little of some unsaturated fatty acid; (4) certainly at least five vitamins.

That one must still speak so tentatively upon such a fundamental question as, "What does the body nutritionally need from without?" is a striking illustration of the rich field which here awaits further research notwithstanding the great advances which have been made during the past three decades.

significance and prospects of such research. Even so, it may be pointed out that the discovery of the vitamins is merely a beginning of this new chapter in the chemistry of nutrition; for research has promptly pressed on from the qualitative finding of the existence of previously unknown factors to the quantitative study of their optimal intakes in nutrition. And just as the discovery of the vitamins shows how to avoid deficiency diseases, so the more comprehensive and quantitative type of research covering whole life-cycles and sometimes successive generations of experimental animals is showing the way to advances in positive health. Thus it has been found that, starting with conditions already normal, the quality of the life process may be improved and the average length of healthy life may be significantly extended by so adjusting the relative proportions of every-day foods as to enrich the dietary in certain of its chemical factors. Not only is life made longer, it is also made richer and more efficient throughout.

There are indications that too greatly "forced" growth may be unfavorable to the longevity of the individual; but within the limits of the normal there is apparently a wide zone of possible improvement in the hitherto accepted averages of nutritional accomplishment. Growth and development can be helped and old age can be deferred in the same individuals and by the same chemical improvements in the nutritional intake. Granted that there are limits beyond which growth and longevity can not both be pushed, there is still a rich field of scientific interest and of service to human welfare in the extension of the period of the prime of life by the further development of this quantitative aspect of the chemistry of nutrition.

We are not primarily concerned with the question whether such chemical improvement in the life process through nutrition will materially change the outward physical aspects of the species or the general nature of its chemistry: both these are probably pretty well stabilized by now and individual differences in physical characteristics are probably chiefly determined by the arrangement of the genes in the chromosomes before birth. What we are learning through research in the chemistry of nutrition is, rather, what things and in what relations to feed into the body in order that it may be helped to maintain a more constantly optimal chemical environment within, and we have reason to anticipate that such chemical improvement of the internal environment may be favorable to further human progress.

At any rate, from now on, nutrition can furnish to food economics a more certain and serviceable guidance than has ever before been available. And with the means of education which now exist we may feel confident that, whether through a planned economy, or through the effect of enlightened consumer demand, or both, this newer knowledge of nutrition will continue to become more and more effective.

To increase the proportion of people who, like many that we have already known, are able to render a longer and more efficient service than the average, and whose services, as Eliot pointed out, are of added value in their more mature years because then the community recognizes them as both experienced and disinterested—this will mean much to civilization as well as to the individuals who will receive this longer lease of healthier life through the advances which are just now being made in our knowledge of the chemistry of nutrition.

SCIENCE AND NUMEROLOGY

By Professor JOSEPH JASTROW
NEW YORK CITY

No one knows who invented the term "Numerology" or when it was first used. The absurd doctrine which goes by that name is in its application an entirely modern device, prompted apparently by the popular success of astrology as a profession, or more accurately, a racket. Some ingenious promoter realized that there was money in the proposition that one is born not under a star but under a number. The original idea in modern numerology is not the technique of calculation—which is an old one—but the notion that the result, which is expressed as a digit, carries with it an entire array of character-traits.

If to the letters in John Doe's name are assigned the numerical value of their position in the alphabet, counting from 1 to 9, and beginning again, thus:

J	O	H	N		D	O	E
1	6	8	5		4	6	5

and the numbers added, namely 35, and the 9's thrown out, the result is an 8; and John Doe is an "8." Being an "8," he has a lot of characteristics such as wishing to manage big affairs, the power to struggle against opposition and in general is a corner-stone in the community. The same hocus-pocus is applied to the date of birth, and endless frills increasingly absurd. This and a mass of similar rubbish makes up the "Numerology" that sells.

Professor Bell had the happy idea of rescuing the word, which is a convenient one, and making it include all the varieties of doctrine, from Pythagoras onward, that ascribe mystic meaning and peculiar power to numbers. From now on that should be the meaning of Numerology, and not the absurd system of reading character and fortune in the arbitrary number-equivalents of names, which represents about the crudest superstition and the lowest cerebration that flourishes in these supposedly enlightened days.

Professor Bell is a distinguished mathematician in the California Institute of Technology. He has issued a book bearing the title "Numerology,"[1] which introduces a distinction that will serve as a permanent classification, and may cause surprise at first, to be followed by complete acceptance of the happy idea. His dictum reads that the (or one) opposite of science is numerology. For false as may be the doctrine throughout, what is now a debased and profane distortion was once a sacred doctrine. This survey of the origin and career of the type of thinking which numerology represents is one of the many merits of Professor Bell's engaging contribution.

Although everybody uses the word, it has not (until recently) been admitted to the standard dictionaries, because it is regarded as a coined word, associated with a very shady system. The libraries have been similarly orthodox; one must look for books on this subject under the title "Symbolism of Numbers."

The father of numerology in the catholic sense, is Pythagoras. Professor Bell, speaking as a mathematician, sets forth:

> It makes not one particle of difference to-day whether a particular mathematical theory of the universe is fantastic nonsense, provided only the theory is of some use for a week or more in guiding scientific work. Because some theory makes correct predictions in three or three hundred instances is no evidence that it is more than a fictitious scaffolding of imaginary and unnecessary lumber. In so far as anyone believes the contrary, to that extent is he a

[1] Eric Temple Bell, "Numerology," Williams and Wilkins Company, Baltimore, 1933.

> numerologist in the traditional sense of the Pythagoreans. . . .
>
> Pythagoras was a pioneer . . . first in each of three fields which we still cultivate, philosophy, science and numerology. A great philosopher, a great scientist, he was also a numerologist, and by the last he is most remembered. . . . Had Pythagoras been invited to define numerology he might have described it as the love of numbers for their own sake.

Pythagoreanism goes farther than this in that it makes the universe an incorporation of the simple relations of numbers; thus does cosmos emerge from chaos. And Pythagoras had a remarkable fact to go upon: namely, the discovery that the relations of the three simple musical intervals, the octave, the fifth and the third, can be expressed by the relations of 1 to 2, 2 to 3 and 3 to 4. "The fact that a beautifully simple relation exists between *small whole numbers* and *musical sounds* astonished and mystified the Pythagoreans, as well it might have done. Few human beings find anything so beautiful. Numerology was born the hour Pythagoras discovered the law of musical intervals. But this "brilliant discovery tricked him into universal numerology." Hence the "harmony of the spheres" and the Pythagorean dictum that all things are fittingly ordered according to the nature of numbers; number is the eternal essence; God is number; number is God. With that the mischief was done; twenty-five centuries later we have the plague of "numerology."

With this start, numbers were assigned moral qualities and forms also. *1* was a masculine number representing unity, and *2* a feminine number, representing discord. The circle was the "perfect" figure; for centuries the orbits of the planets were coerced into more and more complex systems of orbs and cycles until eventually Keppler discarded all this prepossession and found the true orbit in an ellipse. Numbers were perfect and imperfect, odd and even, and assumed qualities similar to the distinction of right and left, which makes sinister—left—equivalent to ill-omened. Plato, continuing the same tradition and in far more mystical fashion, summed it up by saying that God geometrizes. All this is "Numerology," a generic category of pseudo-science.

Numerology developed a numerous progeny, each more bizarre than the others. The system that reached the greatest vogue was of Jewish origin in early Christian days and received the name of the Kaballa. Its technical name was Gematria. The number-scheme by which John Doe finds his numerically equivalent number is a weak form in scriptural texts of Gematria. The Kaballists found mystic meaning by adding up the number-equivalents of names. Gematria is a word admitted to the dictionaries and libraries. To indicate that it still flourishes, I mention a work projected in seven volumes, three of which have appeared since 1932, entirely devoted to numerological texts, from which the truth of the Scriptures and the fate of the great war may be rigidly deduced. So recent and so continuous is numerology. The number of persons who have gone mad on numbers is numberless.

Numerology received its largest theological vogue from the number of the beast in St. John, "666." This text gave rise to what Professor Bell calls "beasting," attributing this malign number to one's favorite enemy. The numerologists of the Reformation fastened it upon Leo Xth, making his name come out "666," and the latter's champions fastened it upon Martin Luther, corrupting his name in the process. By this influence numerology was transferred from science to theology, though it pretends to both sanctions.

Returning to the Pythagorean tradition, it appears that in the course of numerological study important principles and relations of numbers were discovered, not so directly, numerology gave an impetus to mathematics, as astrology did similarly to astronomy and

alchemy to chemistry. All these pseudo-sciences proceed in the same mystical temper, the same devotion to ambitious generalizations, the same slant toward reading character and human fate. It is only in their degenerate forms that they become fortune-telling systems.

Numerology is pseudo-science on the basis of numbers. A typical idea is that of recurrence, the notion of cycles which, transferred to the cosmos, would eventually mean that everything that happens once will in due course happen again. The analogy was found in the recurring decimal 210210210 "To a gin free rationalist it is incredible that brooding on this eternal recurrence could ever have driven an educated man mad, but it did." For in this is the seed of the reincarnations and other doctrines associated with Pythagoras. The numerical side of numerology developed useful knowledge of number-relations, such as primes and squares. But for each discovery there was in turn a misleading numerological setback. Thus 220 and 284 were an "amicable number pair," in that each has the power through its divisors to generate the other.

The popular association of properties and fates with numbers—3's and 7's and again 13's, and all the variety of number superstitions—though they proceed on a history of their own—have a common root in Pythagorean notions, still surviving in the expression: a "square deal," which is both quadrilateral and just. The most famous numerological statement of old is the paragraph of Plato in which he describes the "nuptial" number.

> Whose base, modified, as four to three, and married to five, three times increased, yields two harmonies: one equal multiplied by equal, a hundred times the same: the other equal in length to the former, but oblong, a hundred of the numbers upon expressible diameters of five, each diminished by one, or by two if inexpressible, and a hundred cubes of three.

This mysterious calculation results in the number 12960000; it remained for a woman mathematician, Mrs. Young, in 1923 to discover how Plato derived it.

Derived forms of numerological thought, according to Professor Bell, abound in modern mathematics. Isomorphism is one of them and extrapolation another; and each is an example of the central numerological fallacy which is thus tersely put: a map is not the thing mapped. Identifying the map with what it represents, the number or the name with the thing, thus comes to be the major fallacy of pseudo-science, which I have ventured to call "attributism."

I can not present all the varieties of consequences which Professor Bell derives from his important thesis. I can only promise his readers a number of surprises and a most amusing occupation. The combination of mathematics and humor is rare enough to be noteworthy. In this "Numerology" we have a brilliant exposition of a novel approach to an important chapter in the history of science. "Numerology" is too good a word to lose. In the dictionaries of the future, it will be defined as that form of pseudo-science which ascribes all manner of virtues and qualities to numbers. The several varieties of it, appearing from Gematria to the modern fortune-telling device, will be recognized as so many variations of a great mislead. It will hardly do to allow the last comer, who happened to invent the name, to patent the word merely because he devised the most ridiculous example in the long line of descent. Numerology, as the opposite of science, is a concept of enduring worth to the study of logic.

The problem remains for the psychologist to discover "why our bedivilled race is more receptive to philosophy and numerology than it is to science and arithmetic." And still it is proper to honor Pythagoras.

"And so Ave atque vale, magister Pythagoras, alter ego! Our science is your shadow stripped of its numerology."

SCIENCE SERVICE RADIO TALKS

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CHEMISTRY AND RECENT MEDICAL PROGRESS

By Professor JULIUS STIEGLITZ

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FROM the inception of life to our return to dust, life consists of continuous chemical processes in which matter is transformed for the uses of the body. This close connection between life and chemistry makes chemistry a powerful instrumentality for progress in medicine. In recent years the greatest progress which has followed from this cooperation is undoubtedly in the domain of "the little things"—those extraordinarily powerful principles, the hormones and the vitamins, which are absolutely necessary for the healthful functioning of life. To-day's talk will be devoted to these "little things."

The hormones, produced in our own glands of internal secretion, and the vitamins, produced in plant life and taken into our bodies in our food, are perfectly definite chemical principles or compounds—as definite in their composition and actions as are such well-known drugs as morphine, quinine and strychnine.

Chemistry's first function has been to isolate these mysterious and powerful agents as pure crystalline compounds, exactly as one of its great contributions to medicine in the past was to isolate from crude drugs the pure alkaloids. These pure principles, strychnine, atropine and many others, gave physicians the opportunity of using exact doses to secure perfectly definite effects in place of haphazard results with the crude drugs; it became possible by hypodermic or intravenous injections to obtain almost instantaneous results. We are now witnessing the extension of this mastery of the situation in the use of the hormones.

Thus, epinephrine, or adrenalin as it is more frequently called, is the first of the hormones isolated by chemistry, by that great leader, John J. Abel, of the Johns Hopkins University; its availability in the form of pure crystals has saved many a life. Stillborn babies have been revived by its aid and saved for normal and useful lives. One instance of my own personal knowledge may interest you. A mother lay ready for childbirth in the Chicago Lying-In Hospital but with a heart too weak for the strain. Her heart had actually stopped beating and the eminent obstetrician faced the necessity of sacrificing the mother to save the child by a Caesarean section, but he allowed one minute more for an injection of epinephrine. The heart responded, the mother revived, and with the support of epinephrine passed through her ordeal. To-day both mother and child are alive and well, thanks to Abel and his tool, chemistry, and to up-to-date courageous medical care. Note that these signal services of medicine have been made possible only by the isolation of the pure crystalline principle to be injected in sterile solution hypodermically, intravenously or even into the heart, at the will of the physician. The dose of this powerful drug is one milligram, less than the weight of an ordinary particle of salt.

After isolating a pure hormone chemistry determines what we call the structure of the molecules of the principle—that is, the exact manner in which the atoms of carbon, hydrogen, oxygen and nitrogen are linked together. This the chemist does by a study of the fragments when he breaks up the principle and by a reconstruction of the molecule. The way is thus opened to the artificial preparation of the vital principle. This has been done for epinephrine, and the artificial compound is in common use. This study of the structures of hormones may well lead to attempted improvements on Mother Nature's products. A famous illustration of this possibility is the local anesthetic cocaine, an invaluable aid in surgery. The knowledge of the structure of cocaine led to the artificial drug novocaine or procaine, which is less poisonous than cocaine, very much cheaper and not habit-forming, as cocaine is. Thousands of operations are now carried out under the influence of procaine and similar creations of the chemist. In a similar way man's intelligence may endeavor in time to improve on blind nature's hormones; the results might be of inestimable benefit to individuals and to the race.

Epinephrine is found in the medulla of the adrenal glands, small glands located just above the kidneys. In the last years the attention of medicine has been directed intensively to a second hormone found in the cortex of the gland. Degeneration of the cortex causes Addison's disease, a wasting, hopeless disease. Many chemists and medical men have sought for the active principle of the cortex. In September, 1931, Dr. F. A. Hartman, of the University of Buffalo, electrified the world by describing the effects of a particularly active extract of this principle, which he named cortin. Thus, an apparently lifeless man suffering from Addison's disease was first revived by an injection of epinephrine and then by treatment with cortin restored to partial health within three days. Cortin and similar extracts are now being used regularly in hospitals. There is another side to the story of primary interest to chemists and the public. Cortin is so scarce that the adrenals of 50 to 150 head of cattle yield only a single day's dosage for a sufferer from the disease. The dose costs six dollars. It is vital that chemists isolate the absolutely pure principle and without delay determine its molecular structure so that efforts for its artificial production can at once be started. I understand that Dr. E. C. Kendall, of the Mayo Foundation, has made important progress in this direction.

Kendall, in 1915, discovered the crystalline principle thyroxine, the most important hormone in the thyroid gland, and he demonstrated that a dosage as small as a pinhead a day for a year was sufficient to change a poor little cretin, dwarfed in mind and body, into a healthy, happy and normal child. In 1926, Harington, in London, succeeded in determining the structure of its molecules, and within a year Harington and Barger prepared the hormone artificially. Synthetic thyroxine is now used in medicine.

The story of the hormone insulin, the sugar-consuming principle of our blood, is too well known to require more than brief comment. It has saved and prolonged the lives of hundreds of thousands of diabetics. The pure hormone was finally prepared by the veteran Abel, of the Johns Hopkins University. Unfortunately its molecular composition and structure are very complex. It will probably be a matter of years still before complete chemical knowledge will make possible its artificial preparation and open the way to carefully planned efforts to improve on nature's handiwork. The most pressing need of improvement is to discover a substitute which can be taken by mouth rather than by hypodermic injection.

The most decisive progress has recently been made in the isolation of the sex hormones, especially those of the female organism. There seem to be no less than five of these involved in its characteristic activities. One of these principles is especially well characterized chemically and biologically. It was first isolated by an American, Dr. Doisy, of St. Louis University, who named it theelin. It contains no sulfur or nitrogen and its molecule contains 42 atoms of carbon, hydrogen and oxygen. Injection of such minute quantities as 1/8000 of a milligram (two millionths of a grain) in unsexed white rats develops normal sex activity. As an indication of what the future may hold for us when synthetic chemistry may attempt to modify and improve on hormones, a fact observed by Butenandt is noteworthy, namely, whereas natural theelin is effective in an animal for three days, its benzoyl ester, an artificial derivative, is active for eleven to sixteen days—probably as a result of a slower elimination.

Theelin is already used in medical practise: in a tragic case of dementia following the exhaustion of childbirth, after years of ineffectual treatment and hospitalization, theelin at last has given beneficial results. In a similar fashion extracts of ovarian principles are often found beneficial in relieving the high blood pressure and similar effects of the menopause in women.

The possibilities of hormone therapy in the alleviation of certain mental diseases in general are most encouraging. Dr. Hoskins, of Harvard, has reported that five out of eighteen victims of the dread disease dementia praecox were so benefited by the thyroid hormone that they could be discharged from treatment. The sooner chemistry offers medicine the pure hormones, so that exact therapy can be used, the more rapid will be the progress in offering the body the perfect balance of these powerful agents which our complex organization demands.

The same type of progress in the cooperative efforts of chemistry and medicine is to be observed these days in the field of vitamins. Vitamin A, found in butter and fish-liver oils, has been isolated by Professor Karrer in Switzerland and the structure of its molecule has been determined. There should be no unusual difficulty in preparing it artificially. A Nobel prize was awarded to Professor Windaus, of Göttingen, for the isolation of the pure anti-neuritic principle, vitamin B, as obtained from yeast, and of vitamin D, a specific for the cure of rickets in children. Vitamin C has also been isolated in pure form by two or three independent investigators. The effectiveness of these pure principles is astonishing: thus ergosterol, the chemical precursor of vitamin D, in doses as small as 1/50,000 of a milligram, cures rickets in a mouse—that is, there would be perhaps 200,000 mouse-doses in a particle the size of a grain of salt. All these principles should soon be available for medicine.

In conclusion, let me say that among the great problems facing the alliance of chemical and medical investigation is the discovery of how these powerful principles, hormones and vitamins, bring about in such minute quantities their marvelous effects. Do they start a train of reactions like a match applied to a fuse? Do they unlock doors as keys would? Or do they perchance act like express elevators in skyscrapers, acting on small units, over and over again, with marvelous speed, thus facilitating the transformation of matter which spells life? The field is a fascinating one for scientific investigation and of the very greatest importance for medical control of life and health.

PESTS AWAY FROM HOME

By Dr. L. O. KUNKEL

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DIFFICULTIES with plant disease and insect pests arise whenever anything is done which upsets those wonderful relationships known as the balance in nature. When, for example, a disease or insect pest is accidentally carried from its native home to a new country, it frequently upsets the balance existing among the species in that country. In 1904 a fungus disease known as "chestnut blight" was found to be killing chestnut trees in the vicinity of New York City. From the port of New York, into which it is believed to have been introduced from China, the disease has spread throughout most of the eastern states. In this vast region not a single tree of the native American chestnut is known to have escaped infection, and not one has been found which successfully resists the disease.

It takes about ten years for the blight fungus to destroy all the chestnut trees in any area which it invades. At the present time it is killing the last of the chestnut forests in the southern Appalachian Mountains. Here the chestnut represents about 25 per cent. of the forest stand over an area of about thirty-three million acres. The chestnut trees in this region occupy about 3¼ million acres, and if harvested would yield about fifteen billion board feet of lumber. Millions of dead trees still stand in the forests through which the blight fungus has passed. If harvested promptly these trees yield a good grade of lumber. They may also be used for the production of extract wood, pulp wood and fire-wood. Many of them, however, can not be harvested promptly. They must be left to rot and are a total loss.

As the blight spreads from region to region by means of innumerable tiny wind-blown spores, the question arises as to whether the native American chestnut, *Castanea dentata*, will eventually become extinct. In the mountains of Pennsylvania, and in some other areas where the chestnut did not grow to be very large, a considerable number of trees have not been killed outright. The above-ground portions are dead, but the roots and crowns have remained alive, and have sent up strong shoots. Many of the shoots have become infected and have been killed, but many others, either because they have escaped infection or because they possess a certain degree of resistance to the fungus, have reached considerable size and have borne a few nuts. If some of these nuts should produce trees that are more resistant than their parents, and these trees in turn should give a progeny of varying resistance, it is possible, since only the most resistant would survive and produce nuts, that the species may in time reestablish itself in the forests from which it has been swept. The final outcome of the struggle which the chestnut tree is waging against extinction by the Chinese blight disease can not be predicted at present.

If, now, we inquire into the behavior of this terrible blight fungus in its native land, we find that it does not cause a serious disease of the chestnut trees there. When at home it lives on the Chinese hairy chestnut, *Castanea mollissima*, which is a close relative of the American species. These trees are not badly injured by the disease. This is doubtless because the Chinese chestnut, having been parasitized by the blight

fungus over a very long period of time, has developed a resistance sufficient to hold it in check. The American chestnut, having had no previous contact with the disease, has developed no resistance. Crosses are being made between susceptible American chestnuts and resistant Chinese trees. It may be possible to obtain hybrids having most of the good qualities of the American species combined with resistance to blight shown by Oriental species.

Two years after the chestnut blight was found near New York City, another foreign tree parasite was discovered at Geneva, New York. This pest, which is also a fungus, attacks all the eight species of native American white pines. It is known as the "white pine blister rust." It is impossible to estimate the damage which this parasite has already done. Many pine trees of great commercial value have been destroyed. Many others, occupying large areas of forest lands in thirty-eight states, are threatened with destruction. Millions of dollars have been spent in attempts to hold it under control. Nevertheless, it has spread throughout New England and into the states of New Jersey, Pennsylvania, Michigan, Minnesota, Wisconsin, Washington and Oregon.

Like the chestnut blight fungus, the blister rust spreads by means of small spores. On germination the spores produce the parasite which usually infects twigs at the bases of the leaf fascicles. From the small twigs it grows into the large branches and often into the trunk. It kills the trees by girdling them.

In one important respect this parasite differs greatly from that of the chestnut blight. It belongs to that interesting group of fungi which require two entirely different kinds of host plants. In order to complete its life cycle, the white pine blister rust fungus must find a currant or a gooseberry bush as well as a pine tree on which to live. Spores produced on pines are able to infect currants and gooseberries only. They do not infect other pines. The disease can not, therefore, spread directly from pine to pine. The spores produced on currants and gooseberries infect pines; but if no currants or gooseberries are available, the fungus can not go on to the pine trees. This dependence on currants and gooseberries is a vulnerable point in the life of the blister rust parasite. If all the currant and gooseberry bushes growing in the vicinity of a white pine forest are destroyed, the fungus can not attack that forest. Eradication of these bushes, which are of course less valuable than pine trees, is the most effective means known for controlling the blister rust. The chief weakness of this control measure is the difficulty of finding and killing all currants and gooseberries, especially in regions where these bushes grow as wild plants.

A parasitic fungus, *Tuberculina maxima*, has recently been found attacking the white pine blister rust in this country and in Canada. It lives on the spores of the rust and destroys them. Good results have been obtained in controlling the blister rust in some European forests by means of this natural enemy. It may become effective in holding the disease in check in certain parts of the United States. This example of a parasite living on another parasite furnishes a good illustration of the complicated relationships existing in nature. All living things, including the parasites which cause disease, are subject to attack by other living things.

Those who have studied the rust parasite believe that it, like the chestnut blight fungus, is of Asiatic origin. Instead of coming directly from Asia, as the chestnut blight fungus is supposed to have done, it is believed to have reached America by way of Europe. The blister rust is endemic in western Siberia, where it causes a mild disease of *Pinus cembra*, a pine native to that region. This species, having been ex-

posed to the parasite over a long period of time, has acquired sufficient resistance to hold it in check. Our white pine species are equally resistant to most of the pine diseases endemic in this country. They have, however, had no opportunity to develop resistance to foreign parasites such as the blister rust.

The chestnut blight fungus is believed to have reached this country on chestnut trees imported from China, the blister rust fungus on white pine trees imported from Europe. If these parasites had caused severe and conspicuous diseases in Asia, they might never have escaped from that country. The mildness of the diseases they produce there prevented their being recognized and eliminated from trees selected for export. Both reached this country before the enactment of plant quarantine laws.

These two diseases which have cost us so much indicate how dangerous it is to permit the entrance of foreign plant diseases into this country. Equally striking examples of foreign insect pests which have become established here and are doing much more damage than they were ever known to do in their home countries could be cited if time permitted. A similar behavior might also be described for some of our own native pests which have become established in Europe and elsewhere. Obscure and seemingly harmless insects may, when brought into any new environment, so upset the balance in nature as to become veritable scourges. Likewise, diseases which are almost, if not quite, harmless in their native lands frequently misbehave in the most alarming fashion when taken away from home.

THE EARLY IRISH RACE

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THE racial history of Ireland still awaits serious investigation. Much that was written up to about fifty years ago about Phoenician and Spanish strains in the Irish people was based on insecure legends or even on fancies of the flimsiest kind. This brief talk will not deal with any elements that may have reached Ireland since the fifth century of our era, but will confine itself to the ancient constituents of the Irish people in St. Patrick's time, as far as these can be distinguished at the present moment.

For the earliest period, information can be obtained only from the examination of very ancient skeletal remains revealed by excavations. Such discoveries have been relatively few, for systematic investigation remains to be carried out. Altogether, hardly five hundred skeletons or skulls have been found. But nearly 80 per cent. of the oldest skulls are either long or medium in proportion to the width, and the older the skulls, the greater the percentage of long ones. Broad or round skulls do not appear in any numbers till far into the Christian era. I may note that long skulls are those whose width is less than three fourths of their length; broad or round skulls have a width of four fifths or more of their length; those in between are called medium.

As to stature, the indications are that these early skeletons are short to medium: for men, they vary from 5 feet 5 inches to 5 feet 9 inches; for women, from 5 feet to 5 feet 5 inches. As to the color of complexion, hair and eyes, of course no traces of such traits can be left after a lapse of four thousand years.

The presence in Ireland, two thousand

years before the Christian era, of a medium-statured race with medium-shaped skulls is quite in harmony with the discoveries made on the continent of Europe. There it is found that the "chipped-stone" or "paleolithic" age is associated with long skulls, but that at the end of this period some broad skulls appear, and that during the succeeding "polished-stone" or "neolithic" age these broad skulls become more frequent while also a mixed type, like that of the Irish skulls, appears.

The long skulls are ascribed to the "Mediterranean" race, which is medium in stature and dark in complexion, hair and eyes. The broad skulls are ascribed to what is now called the "Alpine" race, which shows lighter coloration and stockier build than the "Mediterranean," but is like it in its medium stature. At a later period the "Teutonic" race appears, with all the traits attributed by the Greek and Roman writers to the Celts and Germans, namely, a combination of long-headedness with tall stature and fair or reddish hair and blue eyes. By the mixture of the "Mediterranean" and "Alpine" types arose the medium type of skull found in Ireland, as well as in England and on the continent. But the broad skulls found in the latter places are almost entirely lacking in Ireland. In Scandinavia, if Montelius is correct, the oldest burials show round skulls, and the later stone age burials show skulls of the Teutonic type.

Following the two stone ages came the "bronze" age, with weapons, tools and utensils of bronze, which gradually show an increasing perfection of design and especially of decoration. The bronze age is plentifully represented in Ireland, as it is all over Europe. In England, the use of bronze is associated with burials which contain round as well as long skulls. However, there is no appearance of any new type of human beings in Ireland along with this new kind of culture. It seems that the use of bronze was introduced by trade and not by the migration of people of another physical type; not a trace is found of any radical change in burial customs or in the kind of skulls or objects discovered. Besides, bronze had to be imported, or at least the tin that enters into its composition, for tin is not found in workable quantities in Ireland, although copper is. Tin, on the other hand, is plentiful in Cornwall. The people of Cornwall at a later date were called Dumnonii; if these were the same stock as the Irish tribe called Fir Domnann, it is possible that there was a migration from Cornwall to Ireland; but no reason is apparent for connecting this with the introduction of bronze into Ireland. On the other hand, we know positively that there was an extensive trade between Ireland and the continent during the bronze age, for Ireland had easily worked deposits of gold, especially in Wicklow, and certain types of gold ornaments—torcs and crescents—characteristic of Ireland are found scattered over Europe in bronze-age deposits, and even as far east as ancient Troy near the Bosphorus.

The bronze age appears to have lasted in Ireland from about 2000 till about 400 B. C. without any change in the physical make-up of the inhabitants. The use of iron began on the continent about 900 B. C. in the Danube Valley, in what was later called Austria. The first period of the iron age is called the Hallstatt period, and lasted till about 400 B. C. on the continent. But in Ireland nothing resembling Hallstatt implements is found, except occasional objects that stand out in contrast to a purely bronze-age culture. Their presence is explainable by trade. About 400 B. C. began, on the continent, the second phase of the iron age, called the La Téne period. The Irish iron-age objects belong clearly to this second phase of iron-age culture. Consequently, this culture developed in Ireland only after

400 B. C., and we kill two birds with one stone if we postulate the arrival of the Gaels somewhere about the middle of the fourth century B. C.

This supposition harmonizes with what we know of the people called Celts in central Europe. It is commonly accepted that if the Celts of central Europe did not themselves discover the use of iron in the Hallstatt region about 900 B. C., at least their neighbors, the Illyrians, did, and the Celts made good use of the discovery. Their expansion into Gaul probably took place between 900 and 600 B. C. This would explain the finding of iron weapons beside tall, long-headed skeletons buried beneath mounds in the Marne region. In the course of time they were amalgamated more and more completely with the primitive population of Gaul north of the Garonne, which was a mixture of the Mediterranean race with immigrants of the Alpine race, besides having absorbed remnants of the taller, long-headed, stone-age types. The ruling class still retained their tall and fair traits in Caesar's time, for that is how he describes the Gauls, probably referring to the leaders rather than the rank and file of the people.

Around 400 B. C. another period of expansion took place among the Celts of central Europe which brought them into Italy, where they occupied the Po valley, from which they advanced southward, capturing Rome in 390 B. C. The movement went on north of the Alps and seems to account for the presence in northern Gaul and in Belgium of the Belgae, who, according to Caesar, were fiercer and ruder than the Galli of the middle portion of Gaul. Some of the Celts taking part in this second irruption into Gaul seem to have entered Ireland by sea during the fourth century B. C., bringing with them the same types of iron weapons and utensils that characterize the Celts on the continent from about 400 B. C. down. It is not clear whether the Celts who entered Ireland at this time under the name of Gaels—anciently Goidels—were Belgae or men of the same type as the ruling class of the Galli. In any case, they seem to be characterized, in Ireland, by tall stature, long skulls and faces and fair hair—in other words, the Milesian type. There is evidence, however, that the Belgae did send colonies into Ireland, though perhaps at a later time. Ptolemy, the geographer of the second century A. D., gives us a map of Ireland on which are placed the names of certain tribes. On the eastern coast he sets the Manapii, which is also the name of a tribe in northeastern Belgium or western Holland. The name still lives in that of County Fermanagh. Several other names given by Ptolemy resemble those of tribes situated near the Rhine, some of them being perhaps German tribes.

These observations do not contradict the descriptions of races that we obtain from the old literature of Ireland. There we find frequent references to the tall stature, the long yellow, golden or reddish hair and the gray or blue eyes of the ruling classes and their heroes, in sharp contrast to the short, stocky build and the short black or brown hair of menials or of enemies in general. This shows the contrast between the Gaels and their predecessors in the country as seen by the Gaels themselves.

As to the Picts, the question is even more difficult. They were recognized as distinct in northeast Ulster up to the eighth century A. D., and were assigned also to several other parts of Ireland where they did not retain their identity. They also occupied Scotland, where they are mentioned historically from about 400 A. D. But as to their description and their relationship to the Caledonians and other peoples, we are entirely in the dark. Another race men-

tioned is that of the Fomoraigh, described as black, and as making piratical incursions from the north by sea. All we can say about them is that their name seems to mean "seashore folk" and to be the etymological equivalent of the German Pommern, which is from the Slavic Pomoře, meaning also "seashore folk." The Tuatha Dé Danaan, "the tribes of the goddess Danu," are a mythical people who really represent the gods of the old Pantheon; though it is true that some early writers describe them as a race of men.

As to the language spoken by the various racial types that go to form the Irish people, there is no evidence of any other language but Gaelic being spoken in Ireland during the historical period. In the old stories representatives of the various racial groups converse in friendly or in hostile fashion without any suggestion of a difference of language or even of dialect. In the case of the Picts of northern Scotland, there is mention later on that the missionary Columkille and his disciples had need of interpreters when they went to preach to them; while in southern Scotland—where the Picts were closely associated with the Scots, as the Gaels were called—there appears to have been no such need. There are also some inscriptions found in Scotland, thought to be Pictish, which have not yet been interpreted. In Ireland, no remains of a non-Celtic language are found, except in certain geographical names, like that of the Shannon River and some others. Does this indicate that the predecessors of the Gaels already spoke a Celtic language when they arrived in Ireland? I think not, for the pre-Gaelic population goes back without any serious interruption or change to the polished-stone age, at which time Celts are unknown, even in central Europe. The same may be said of the shorter element among the Celts or Gauls of the central portion of Gaul in Caesar's time, in spite of the fact that some ethnologists apply the name "Celtic" to the round-headed "Alpine" race which during the polished-stone and bronze ages mixed with the earlier long-headed "Mediterranean" race to produce the medium-headed, medium-statured, medium-colored race that is so much like the basic element in Ireland. There is no evidence that there were any Celts in existence at that time, nor at any time before 900 B. C. In the proper use of the term, the "Celts" are those who used a Celtic language; and the earliest descriptions we have of a people speaking a Celtic language make them tall and fair like the earliest Germans. While it is possible that the pre-Celtic population of western Europe used some other form of Indo-European language, we do not yet know what language was spoken either in Gaul or in Ireland before the coming of the historical Celts, into Gaul about 900 B. C. and again about 400 B. C., and into Ireland a little after 400 B. C. But these speakers of Celtic spread their language over the populations they subdued, and so completely that only the barest traces remain of any older language. In both cases a homogeneous people was produced, not indeed in the physical or anthropological sense, but in a linguistic, psychological, cultural and national sense. National unity rests, not on a physical basis, but on the basis of a common culture, a common historical development and a common ideal of life.

NOCTURNAL DREAMS

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SCIENTIFIC data pertaining to dream phenomena are admittedly inadequate. The paucity of experimental studies upon the problem of dreams is chiefly due to the difficulty of applying experimental techniques; a scientific investigation of dreams by either objective or introspective methods is for obvious reasons exceedingly difficult. The content of dreams has been studied traditionally by the introspective and the questionnaire methods. Bentley,[1] for example, has achieved some rather meager, though valuable, results from an introspective technique, which he describes with considerable confidence. Introspection here, however, as elsewhere, has many limitations and imperfections. "It is almost impossible," suggests Morgan, "to get an experienced observer to narrate a dream without interpolating explanatory and organizing material."[2] Moreover, a study of dream content by introspection is largely a study by retrospection. Accordingly, Calkins remarks: "The difficulty of remembering dreams suggests, of course, the impossibility of an exhaustive enumeration of their peculiarities and any positive conclusions from the figures. . . ."[3]

The present discussion attempts neither to set forth an experimental technique nor to suggest a theoretical approach to the problem of dream content; nor is it concerned with the etiology of dreams. It merely deals with the reporting of a few interesting facts derived from a questionnaire-census of the ordinary dream experiences of a limited number of unselected college students, enrolled in various psychology courses. Originally, this material grew out of a much more comprehensive survey, which dealt with psychopathic and psychoneurotic tendencies, and sleep phenomena other than dreaming. Only those items pertaining to nocturnal dreams will be considered here. The subjects used in this study comprised 170 college students—81 men and 89 women. All of them had sometime previously been given two class lectures on the limitations and the inadequacies of subjective methods of securing psychological data, and they had been properly advised of the necessary precautions to be taken in the use of such methods. Furthermore, a majority of the subjects had been instructed previously in one of their psychology courses concerning the special limitations of the questionnaire method of investigation. Thus, every possible step was taken to prevent any unnecessary abuse of the techniques to be used.

The subjects were first informed by the examiner of the nature of the questionnaire. They were repeatedly told that the data were to be used for statistical purposes only. They were urged to use the utmost caution, and they were instructed that, in no case, were they to answer any question unless they were fairly positive that their answers were correct. Finally, they were assured that their answers would be kept strictly confidential, inasmuch as their names were not to be affixed to their papers.

The general procedure was as follows: The questionnaire items were dictated by the examiner instead of being distrib-

[1] M. Bentley, "The Study of Dreams; A Method Adapted to the Seminary," *Amer. Jour. Psychol.*, 26: 196–210, 1915.

[2] J. B. Morgan, "The Psychology of Abnormal People," p. 406. New York: Longmans, Green and Company, 1928.

[3] M. W. Calkins, "Statistics of Dreams," *Amer. Jour. Psychol.*, 5: 312, 1892–93.

uted in mimeographed form. Six blank sheets of paper were placed in the hands of each subject. At the top of the first blank sheet the subject was told to signify his age, collegiate classification and sex. The items of the questionnaire were then dictated by the examiner, each item being read twice. The subjects were carefully instructed not to answer any question until they were told to do so, and from one to two minutes were allowed for each questionnaire item.

The subjects gave their answers to each question in accordance with the specific instructions of the examiner, and at a specified time. For each questionnaire item the examiner took sufficient time to clear up any ambiguous terms or misconstrued meanings. In spite of such careful instructions, however, several subjects failed in many instances to respond according to directions (as is usually the case in a questionnaire survey of this kind), and their answers had to be ignored statistically.

The questionnaire items appear below, but space does not permit the statistical tabulation of results, with responses for each sex and totals for both sexes.

THE QUESTIONNAIRE

1. (a) Do you dream?
   (b) About how much?
2. Do your dreams appear to be simple or complicated?
3. (a) Do you have fear dreams?
   (b) About how frequently?
4. Are you usually an active participant in your dreams?
5. Would you say that your dreams usually contain some element from the experience of the previous day?
6. (a) Do you have dreams which seem to "come true?"
   (b) To what extent?
7. Do you have dreams of soaring, falling, or flying?
8. (a) Do you have dreams of frustrated effort?
   (b) How common are such dreams?
   (c) Would you judge yourself as having an inferiority complex?
   (d) Do your dreams of frustrated effort seem to be related in any way with your inferiority complex?
9. (a) Do you ever dream of being nude?
   (b) How common are such dreams?
   (c) Does this kind of dream seem to cause you any special embarrassment?
10. (a) Do you sometimes dream of a dead person returned to life?
    (b) Are these dreams inclined to be recurrent?
    (c) Do you usually dream of the same person?
11. (a) Do you sometimes dream that some dead person has never died?
    (b) Are these dreams inclined to be recurrent?
    (c) Do you usually dream of the same person?
12. (a) Do you sometimes dream of the death of some relative or friend, now living?
    (b) Are these dreams inclined to be recurrent?
    (c) Do you usually dream of the same person?
13. (a) Is visual or auditory imagery more frequent in your dreams?
    (b) Do you get any color imagery in your dreams?
14. Are your dreams usually pleasant or unpleasant?
15. Do you believe that you do any thinking in your dreams?
16. Do your dreams usually seem to take up considerable time?
17. Do you believe that you ever remember any dreams which do not cause you to wake up?
18. Are you inclined to worry much about your dreams?
19. Do any of your dreams seem to be influenced by strong desires?
20. Are your dreams usually absurd?
21. Are your dreams dismissed from your mind easily and quickly, or is there a tendency to remember them for some time?
22. (a) Are you inclined to tell your dreams to friends?
    (b) Does the mere "telling" seem to give you a feeling of relief or satisfaction?
23. Do any of your dreams seem to contain a feeling of future significance?
24. Did you ever have a dream of paralysis?
25. (a) Do you have recurrent dreams?
    (b) How regularly?
26. Would you say that any of your dreams appear as an imaginary fulfillment of a repressed sexual wish?
27. Are you ever able to trace any of your dreams back to some special stimulation of the body surface during sleep?
28. Do your dreams often violate your moral principles?

29. Do you consider any of your dreams as in any way supernatural?
30. Do your dreams ever seem to result from prolonged anxiety?
31. (a) Do you day-dream a great deal?
    (b) Does the content of your day-dreams seem to be closely related to the content of your nocturnal dreams?
32. Do you think that any of your dreams have ever produced in subsequent waking activity any of the following:
    (a) Any prolonged moods?
    (b) Any false recollections?
    (c) Any fears?
    (d) Any delusions?
    (e) Any obsessions?
    (f) Any conduct disorders?

Only a very meager portion of the results obtained from the foregoing questionnaire can be reported here. In general, the data demonstrated that there was not any one essential aspect or attribute common to the dream activity of all individuals—a feature that must always be present. Each dream seemed to be unique and individual in some particular aspect or aspects. The matter of individual differences was, therefore, very apparent; there were great individual differences among individual dreamers.

The data also indicated that sex differences, if they existed at all, were not sharply enough differentiated to be considered very significant. Twice as many men as women reported that they dreamed of being nude; they had such dreams much more frequently and the dream of nudity caused them much less embarrassment. Women were inclined to tell their dreams to friends almost twice as much as men. About twice as many men as women reported that their dreams appeared to be an imaginary fulfillment of a repressed sexual wish. More men than women also reported that their dreams often violated their moral principles. These few differences, mainly of a sexual character, were clearly evident; but the number of cases (170) was so few that it would be extremely hazardous to draw any definite conclusions as to sex differences from such limited data. The general trends are suggestive, but there is the possibility that, if the cases were greatly increased, the sexes might show considerably less differentiation—perhaps no differentiation at all.

The data demonstrated certain other general tendencies, however, and some of the more interesting trends indicated by the questionnaire were:

(1) The subjects were almost unanimous (99.4 per cent.) in saying that they dreamed, but they differed greatly in the amount of dreaming. The assumption that sleep is usually dreamless is certainly not borne out, and, as Fisher has suggested, "is really without the slightest factual basis to support it."[4] De Sanctis and Neyroz[5] have, in the opinion of the author, presented very logical data for postulating a continuity of mental activity during sleep.

(2) Sixty-nine per cent. (69.4 per cent.) of the subjects reported that their dreams were complicated rather than simple. This might be expected of adult dreams, although, according to one investigator,[6] the dreams of children appear to be very simple and direct, consisting often of mere imaginary wish fulfillments.

(3) Ninety-two per cent. (92.9 per cent.) of the subjects reported that they were usually active participators in their dreams. This reveals nothing new or unusual. In fact, it is generally contended that a child is practically always a very active participant in the dream situation—an adult, only a little less so.

(4) Only a very small majority (51.2 per cent.) of the subjects reported that their dreams usually contained some element from the experience of the previous day. This does not support the

[4] V. E. Fisher, "An Introduction to Abnormal Psychology," p. 379. New York: The Macmillan Company, 1929.

[5] S. De Sanctis and U. Neyroz, "Experimental Investigations concerning the Depth of Sleep," *Psychol. Rev.*, 9: 254–282, 1902.

[6] C. W. Kimmins, "Children's Dreams," New York: Longmans, Green and Company, 1920.

wide-spread belief of those who say that a dream invariably contains some aspect of the previous day's experience (the day followed by the night of the dream), although as some observers have noted, this may be true in the dream experience of the child.

(5) Eighty-nine per cent. (89.5 per cent.) of the subjects reported that they had dreams of frustrated effort, though these dreams were not especially frequent. This questionnaire might indicate that such dreams are perhaps more common than is generally supposed, particularly among many types of psychoneurotic personalities. Indeed, a very careful analysis might possibly indicate that a large percentage of all our nocturnal dreams should be classified, basically and fundamentally, as dreams involving the element of frustrated effort.

(6) Thirty-three per cent. (33.9 per cent.) of the subjects indicated that they had dreams of being nude, though such dreams were not especially frequent. The subjects were about equally divided upon the question of embarrassment caused by a dream of nudity. These data may indicate that the great stress upon "utter lack of embarrassment" accompanying such dream experiences, which has been so much emphasized by certain modern writers, is probably exaggerated and has no real factual basis.

(7) Ninety-six per cent. (96.3 per cent.) of the subjects reported visual imagery to be more frequent in dreams than auditory imagery. This much greater proportion of visual imagery supports the findings of practically all investigators of dream phenomena. Calkins,[7] for example, found visual images to be related to auditory in the ratio of 3 to 2. Bentley[8] also found visual images slightly more frequent in the dreams of his subjects than auditory—visual 59, and auditory 51. Morgan summarizes the literature on the subject in the following generalization: "Investigations have shown that by far the greater proportion of dreams are visual. Less than half as many are auditory."[9]

(8) Seventy-four per cent. (74.4 per cent.) of the subjects reported that they did not get any color imagery in their dreams. In like manner, Bentley[10] found that colors appeared in the dreams of his subjects occasionally (11) but that gray imagery was the rule (48).

(9) Sixty-six per cent. (66.6 per cent.) of the subjects were inclined to the belief that they did thinking in their dreams, which is a conclusion quite contrary to the generally accepted view that all thought processes are in abeyance during sleep. These data seem to be in agreement with the findings of Bentley, who comments: "We are told that 'people' do not think in dreams, that dreams are wholly irrational, that incongruities are not recognized, that dreams do not pursue a given topic, and what not. In our own introspections we found not one of these generalizations to hold."[11] However, Bentley is careful to add that "thinking is rare and, as a rule, ineffective."[12] To be sure, Coleridge, according to his own account, got his inspiration for *Kubla Khan* in an opium dream; and Tartini, the celebrated Italian violinist, is said to have told Lelande that he composed the sonata, *Ill Trillo del Diavolo*, or "The Devil's Sonata," as the result of a dream. Likewise, there are many reports of difficult problems having been solved during dream activity, but one is necessarily led to conclude that thinking is very ineffective in the dreams of most of us.

(10) A majority of the subjects indicated that their dreams were inclined to be pleasant rather than unpleasant. This affective element of pleasantness is in keeping with the Freudian interpretation of dreams. However, it does not

[7] *Op. cit.*, 321.
[8] *Op. cit.*, 202.
[9] *Op. cit.*, 405.
[10] *Loc. cit.*
[11] *Op. cit.*, 208–209.
[12] *Op. cit.*, 209.

bear out the investigation of Bentley,[13] who found unpleasant dreams to appear about twice as often as pleasant ones.

(11) Seventy-four per cent. (74.8 per cent.) of the subjects reported that dreams were dismissed from their minds (forgotten) easily and quickly. Morgan suggests that "this tendency to forget is not confined to unpleasant dreams but may affect pleasant ones or indifferent ones as well."[14]

(12) Forty-four per cent. (44.6 per cent.) of the subjects reported that dreams had produced prolonged moods in subsequent waking activity. This tendency has been clearly recognized by Conklin, who remarks: "The emotional accompaniment of a dream often carries over into the day following and serves to produce a mood of considerable duration."[15] This may be true especially of psychoneurotic personalities, who are ever inclined to confuse their dream past with their waking past.

(13) Thirty-six per cent. (36.4 per cent.) of the subjects reported that dreams had produced false recollections in subsequent waking activity. It does not seem at all unreasonable that this should happen rather frequently. "It is highly probable," says Sully, "that our dreams are, to a large extent, answerable for the sense of familiarity that we sometimes experience in visiting a new locality or in seeing a new face"[16] (paramnesia).

[13] *Op. cit.*, 203.

[14] *Op. cit.*, 409.

[15] E. S. Conklin, "Principles of Abnormal Psychology," p. 329. New York: Holt, 1927.

[16] J. Sully, "Illusions," p. 274. New York: D. Appleton and Company, 1897.

(14) Only a very small number of the subjects (5.9 per cent.) reported that dreams had produced conduct disorders in subsequent waking activity. This percentage may be slightly too high, inasmuch as some of the subjects were possibly confused as to the exact meaning of "conduct disorder." However, that such a thing is possible would, of course, not be denied. Prince[17] describes a very interesting case, reported by Dr. G. A. Waterman, in which, under complete hypnosis, an instance of kleptomania could be traced back to a dream which had occurred one and one half years previously.

As has been emphasized throughout, this questionnaire report is in no sense extensive enough to base conclusions upon, but it does point out to the student of dream phenomena certain interesting trends and tendencies, which, for the most part, have been substantiated both by one's own experience and by the investigations of a few experimenters. The chief methods available for clearing away popular superstitions and fallacies relative to dream content would seem to be (1) the extensive study of large numbers of children and adults by the questionnaire-census method; and (2) the intensive introspective study of sufficient numbers of subjects under controlled conditions. In any event, the author believes that the questionnaire-census, with all its serious defects and limitations, has some value in the statistics of dreams, when used to supplement the data procured by other more refined methods.

[17] Morton Prince, "Coconscious Images," *Jour. Abn. Psychol.*, 12: 307–310, 1917.

"PEARL" FARMING IN JAPAN

By Dr. W. O. BLANCHARD
PROFESSOR OF GEOGRAPHY, UNIVERSITY OF ILLINOIS

FIG. 1. CULTURE PEARL FARMS

Japan Must Needs Look to the Sea

The presence of a dense and rapidly growing population in a land of meager resources has compelled the Japanese to carry on an unusually thorough exploitation of the surrounding seas. To this end they have built up one of the world's greatest merchant fleets, they maintain a powerful navy, and they, of all nations, are most dependent for a food supply upon the fisheries. Fish rank next to rice in their diet and the annual per capita consumption of 82 pounds gives them undisputed first rank as the world's greatest consumers of sea food.

Of the various activities designed to wring the utmost of sustenance from her waters, none is more unique or interesting—none, indeed, more spectacular—than the recently perfected method of producing on a large scale the so-called "culture pearls." The story of this accomplishment adds another interesting chapter to the series of triumphs of the scientist in his effort to improve on the crude and ofttimes wasteful methods of nature.

Pearls as the Fisherman's Quarry

Until a little over a decade ago, the genuine oyster pearls of commerce represented the combined harvest from thousands of pearl divers working in the out-of-the-way corners of the tropic and sub-tropic seas. The bulk of them came from the coastal waters off Asia, in particular those reaching from the Red Sea around to Japan. Like most returns from hunting and fishing, both

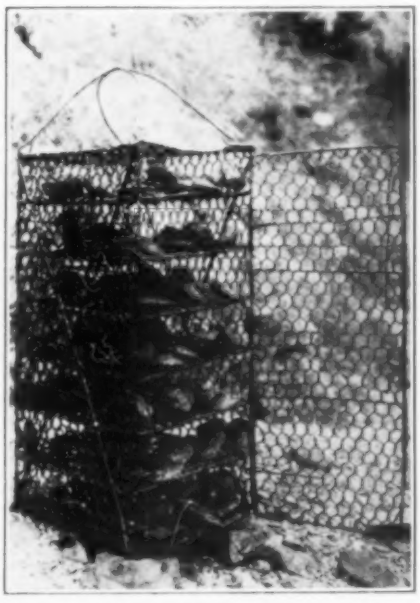


FIG. 2. CAGES WITH OYSTERS AFTER THE OPERATION
READY TO BE LOWERED INTO SEA FOR THE GROWTH OF PEARLS.

FIG. 3. AGO BAY

SHOWING THE BUILDINGS WHERE THE OPERATIONS UPON OYSTERS ARE PERFORMED.

the quantity and quality of the harvest were subject to marked fluctuations, and unrestricted exploitation had in many sections practically exterminated the oyster beds.

It remained for a Japanese scientist to remove the large factor of uncertainty in the pearl supply and to shift the industry into the category of man-controlled operations whose output could be regulated to suit the market needs.

A Japanese Scientist Attacks the Pearl Problem

The evolution of the cultivated pearl industry represents the life work of Baron K. Mikimoto, the Japanese scientist upon whose researches the industry rests. Born in Toba, in southeastern Japan, where pearl fishing was the chief means of livelihood, he early became interested in the improvement of the gems by the selection and cultivation of the pearl oysters. By 1894 he had succeeded in producing semi-spherical pearls attached to the shells, and 9 years later, he was able to exhibit perfectly formed pearls. Shortly after the world war he began to market them, and each succeeding year has seen "Mikimoto pearls" playing a larger rôle in the world's pearl trade.

Requirements of the Pearl Oyster

The requirements for the successful growth of the pearl oyster are rather exacting. The temperature of the water should preferably be above 68° F. (20° C.)—a drop below 50° F. (10° C.) being fatal. Furthermore, the waters should be clear and calm, the bottom clean, and there must be sufficient circulation to bring food and to remove waste. One of the most serious difficulties is the destruction of the oysters by their natural enemies—especially the octopus and starfish, so that freedom from these pests is necessary. Even if these conditions are all complied with, left to themselves, only a small fraction of the oysters actually produce pearls. Consequently, the interference of man is necessary, and we have the final important requirement of a cheap and efficient labor supply.

Geographical Distribution of Pearl Farms

The waters of southern Shima, including Ago Bay, in southeastern Japan, are remarkably clear, the few tributary streams are almost devoid of sediment, and the bottom of the bay is sandy. Furthermore, the south end of the bay is open to the warm Kuro-shiwo or Blue Current, which flows north from Formosa and Luzon. Here Mikimoto has leased the island of Tahoku and has also acquired the rights to about 50 miles of the adjacent waters. Here he established his first "pearl farm." Since then, the work has expanded to include the other sites as shown on the sketch map, but Ago Bay remains the main center of the industry.

How the Pearls Are Produced

Briefly put, the steps in pearl production are essentially as follows:

(1) Young oysters or "spats" about three years old are collected from the sea bottom in 30 to 40 feet of water by a crew of diving-girls (see Fig. 3). The great demand for "spats," with the consequent depletion of supply, has led to an ingenious method whereby the floating larvae are now captured and the spats raised.

(2) These young oysters are taken to a near-by building, where they are subjected to a very delicate operation by highly skilled technicians. This operation—the most critical step in the whole industry—consists in removing from one oyster the mantle parenchyma—a small sack—into which is inserted a tiny bead of mother of pearl. The mouth of the bag is then tied and the sack is inserted into the subcutaneous tissue—the shell secreting epidermis—of a second oyster through an opening surgically made. The cord is then removed, the wound disinfected and the oyster replaced in the sea to eventually cover the bead nucleus with several layers of nacre. For protection against its ene-

FIG. 4. CLEANING THE OYSTERS

TWICE A YEAR THE CAGES ARE RAISED AND THE OYSTERS CLEANED OF MUD AND VEGETATION.

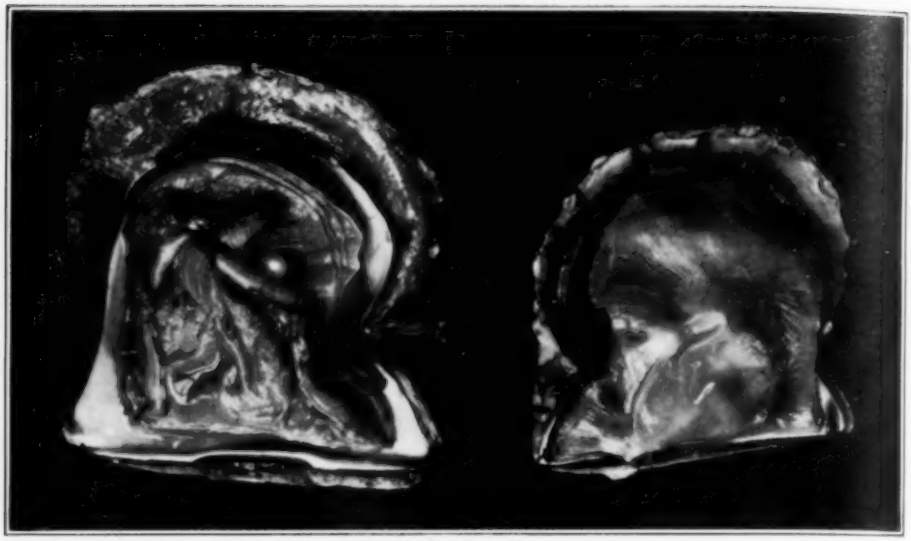


FIG. 5. A 10-YEAR OLD OYSTER
SHOWING A CULTURE PEARL IN PLACE.

FIG. 6. DRILLING PEARLS
FOR STRINGING AT TOBA, JAPAN, NEAR A PEARL FARM.

mies, sudden changes in temperature and muddy water, these potential pearl makers are placed in wire cages and suspended from floating rafts, about 6.5 feet below the surface. When unfavorable conditions develop, such as storms or changes in the current, the rafts are floated into protected waters. Experience has shown that, on the average, they are compelled to move about twice a year.

(3) The oysters live in these cages for seven years, being drawn up once or twice a year and the shells cleaned of weeds and barnacles which hinder growth and sometimes even kill the oyster.

(4) At maturity, *i.e.*, at the age of 10 years, the oysters are removed from the cages, the pearls recovered, cleaned, sorted, and drilled for stringing. The oysters themselves are tasteless and are not used for food. However, the shells are used, part of them for the beads to be used for further pearl production.

The Pearl Harvest

For several years an average of about 3,000,000 oysters were set to work annually, but in 1930 this was increased to 5,200,000 and in 1931 to 6,300,000. Experience has shown that about 20 per cent. die, principally as a result of the operation; a like number fail to produce pearls, leaving about 60 per cent. yielding pearls of various grades. Of these latter only the most perfect are sold under the name of "Mikimoto Pearls." About 600 pounds of pearls are now produced yearly.

The initial offering of these pearls produced a furore in the gem trade. Possessors of pearl stocks had visions of a flood of cultivated pearls which would demoralize the market and ruin prices. Legal steps were taken to bar the new product as an artificial imitation. Experts, however, found that there was no essential difference, that only by making a cross section could one be identified, since that would reveal the bead nucleus.

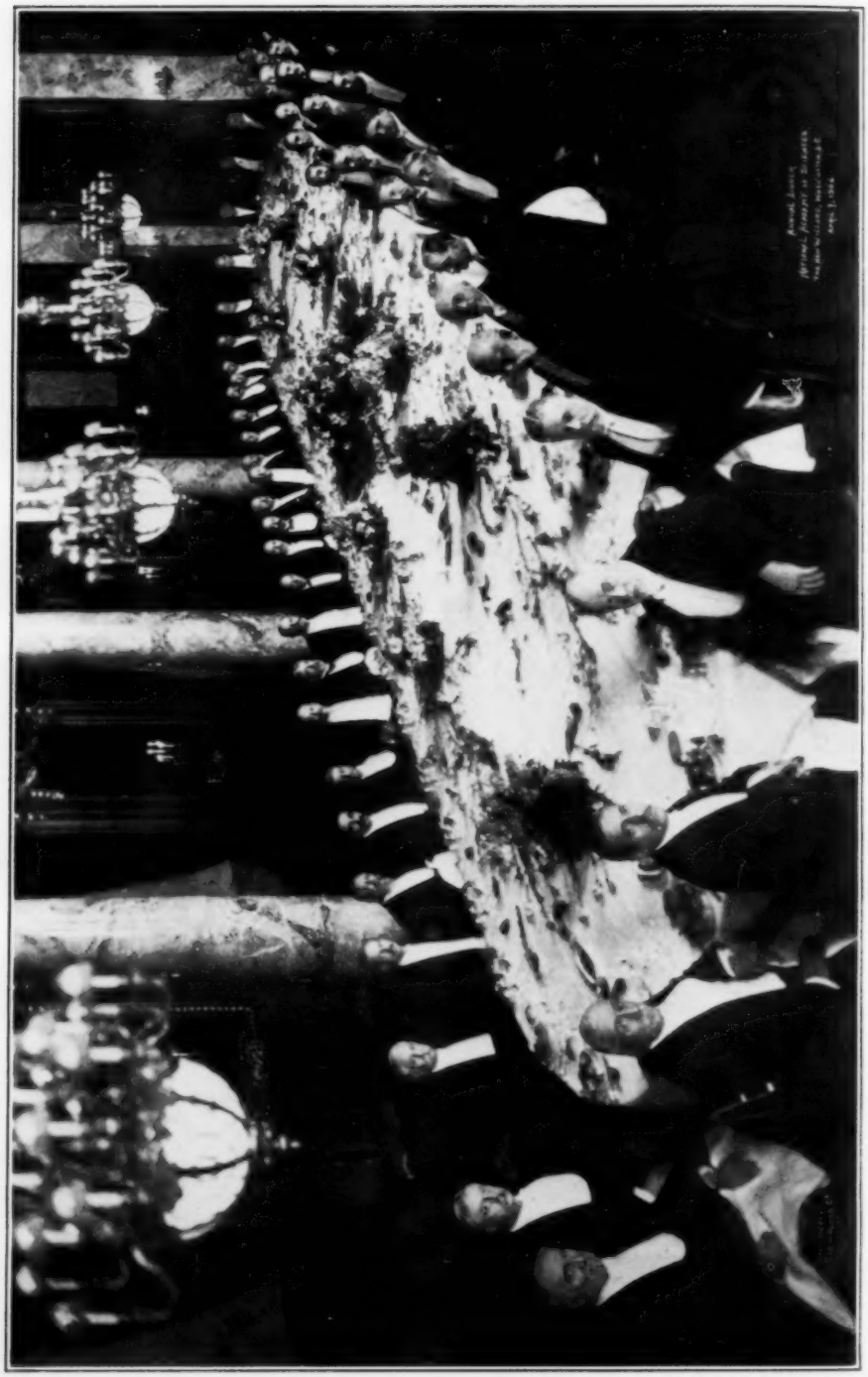
Extent of the Industry

At present pearl farms to the number of ten are located as shown in Fig. 1. Their combined area totals about 40,830 acres, controlled under fishing licenses and leases. The industry employs about 1,000 workers and uses some 80 buildings. With the exception of a small output by lessees of Mikimoto's patents, the whole industry—production, manufacture, and sale—is in the hands of this one firm. They own some 17 patents covering the culture, cages, attachment of larvae, and even the regulation of the luster of the pearls.

Importance of the Human Factor

Undoubtedly there must be vast numbers of waters in our tropic and subtropic seas as favorable for pearl oysters as those used. However, the protection of the many patents, the efficient closely knit corporation with its long experience and highly trained corps of workers, the abundant labor at a low wage scale[1] together with the long period of time which must elapse before the investment bears returns—these would seem to make the likelihood of successful competition by others remote. It is an excellent illustration of the dominant importance of the human over the environmental factor.

[1] The U. S. Bureau of Fisheries experimented with artificial pearl culture in fresh-water mussels and found that while possible, the expense involved made it commercially unpracticable. At the time of writing, March, 1932, the young women employed as divers for spats received an average of one yen, or 32 cents per day.



ANNUAL DINNER OF THE NATIONAL ACADEMY OF SCIENCES, WASHINGTON, APRIL 7, 1906

ALEXANDER AGASSIZ, THEN PRESIDENT OF THE ACADEMY, IS PRESIDING. ON HIS RIGHT IS ELIHU ROOT, ESQ.; DR. WILLIAM H. WELCH; HENRY CABOT LODGE, THEN SENATOR FROM MASSACHUSETTS. ON HIS LEFT IS JOSEPH G. CANNON, THEN SPEAKER OF THE HOUSE; JOHN S. BILLINGS, THEN DIRECTOR OF THE NEW YORK PUBLIC LIBRARY, AND ROBERT S. WOODWARD, THEN PRESIDENT OF THE CARNEGIE INSTITUTION.

THE PROGRESS OF SCIENCE

THE SCIENCE ADVISORY BOARD

By his Executive Order No. 6238 President Roosevelt has appointed a Science Advisory Board to consult with the United States Government on matters pertaining to science. The official communication reads:

The National Research Council was created at the request of President Wilson in 1916 and perpetuated by Executive Order No. 2859, signed by President Wilson on May 11, 1918. In order to carry out to the fullest extent the intent of the above Executive Order, there is hereby created a Science Advisory Board with authority, acting through the machinery and under the jurisdiction of the National Academy of Sciences and the National Research Council, to appoint committees to deal with specific problems in the various departments. The Science Advisory Board of the National Research Council will consist of the following members who are hereby appointed for a period of two years: Karl T. Compton, *chairman*, president, Massachusetts Institute of Technology; W. W. Campbell, president, National Academy of Sciences, Washington, D. C.; Isaiah Bowman, chairman, National Research Council, and director, American Geographical Society, New York City; Gano Dunn, president, J. G. White Engineering Corporation, New York City; Frank B. Jewett, vice-president, American Telephone and Telegraph Company, and president, Bell Telephone Laboratories; Charles F. Kettering, vice-president, General Motors Corporation, and president, General Motors Research Corporation, Detroit; C. K. Leith, professor of geology, University of Wisconsin; John C. Merriam, president, Carnegie Institution of Washington; R. A. Millikan, director, Norman Bridge Laboratory of Physics, and chairman of the Executive Council, California Institute of Technology.

Since its establishment by an Act of Congress, approved by President Lincoln in 1863, one of the important functions of the National Academy of Sciences has been to serve as official adviser to the Government in scientific matters whenever called upon by any of its departments. Specifically, Section 5, Article 5, of the academy constitution reads: "The advice of the academy shall be at all times at the disposition of the Government upon any matter of science or art within its scope." The Act of Incorporation further states that "the academy shall receive no compensation whatever for any services to the Government of the United States."

At the time of its foundation during the Civil War, as the earlier records of the Academy indicate, its committees and its members dealt actively with military and naval problems of precisely the same type as those which insistently pressed for solution during the World War. Similarly, during the World War, the Academy offered its services to the Government and at the request of President Wilson created the National Research Council as the active agent of the Academy to assist the Government in organizing the scientific resources of the country for its need at that time.

As the work of the National Research Council progressed, it became evident that the Council might be a useful organization for peace time and the President's recognition of this led him to perpetuate it to stimulate scientific research, to survey the larger possibilities of science, to promote cooperation in research, to study military and industrial problems in connection with the war, and to gather, collate and make available the scientific and technical information of the world.

The Council is not an institution for the maintenance of scientific laboratories. It is rather an organization which, while clearly recognizing the indispensable value of individual investigations, hopes particularly to help to bring together scattered work and workers. It wishes to coordinate scientific research in America, especially, perhaps, those problems which depend for successful solution on the cooperation of several or many workers and laboratories. A feature of special significance in

the plan of organization of the National Research Council is provision for bringing together year by year outstanding research men as chairmen of the technical divisions of the Council. They are placed in a position to devote their main attention to the study of the conditions of research in their respective fields and have been a means for the encouragement of research in these fields.

Among the various methods of contributing practical assistance to American science in harmony with the general point of view outlined above, which the Council has so far adopted, perhaps the most important is the establishment of special committees of carefully chosen experts for specific scientific subjects or problems urgently needing consideration. They plan modes of attack and undertake to find men and means (with the assistance of the general administrative offices of the Council) for carrying out the plan. Such problems may lie either in the field of pure science or in that of applied or industrial science.

The Science Advisory Board has met twice and at its recent four-day session in Washington it gave consideration to three general problems.

The first was questions of proper organization, or functioning, or program of the scientific and technical services of the Government on which the advice of the board has been specifically requested. The second was similar matters which have otherwise come before the board, and which need attention in order that essential technical services shall not be impaired by economy, or unwise projects be supported, as may easily happen when the determining issues are obscure or highly technical. The third was basic considerations of the more permanent policy of the Government toward scientific work.

President Compton, chairman of the board, states that, inasmuch as the actual problems now under consideration are pressing and of a confidential nature, no discussion of them would be appropriate at this time.

THE BUILDING OF THE NATIONAL ACADEMY OF SCIENCES

—*Bachrach*

PRESIDENT JAMES BRYANT CONANT OF HARVARD UNIVERSITY

NEWLY ELECTED UNIVERSITY PRESIDENTS

At the opening of the academic year newly elected presidents of three leading eastern universities have taken up their work. They are Dr. James Bryant Conant at Harvard University, Dr. Harold Willis Dodds at Princeton University and Dr. Harry Woodburn Chase at New York University.

Born in Dorchester, Massachusetts, in 1893, Dr. Conant entered Harvard College in 1910 and has been identified with this institution since that time as student, instructor, assistant and associate professor, Sheldon Emery professor of organic chemistry, chairman of the chemical division and now finally as president. He is only five years older than Charles William Eliot, also a professor of chemistry, when the latter assumed the presidency in 1869 at the age of thirty-five. He was granted two temporary absences from Cambridge; during the war he responded to a call from Washington to engage in chemical research and organization for the United States Government; in 1925 he devoted much of his time to studying the methods of research and instruction in German universities.

Dr. Conant has distinguished himself by his contributions to organic chemistry. His important work on photosynthesis in plants, published in 1932, earned for him the William H. Nichols medal of the American Chemical Society and the Chandler medal of Columbia University. Dr. Conant has been interested in the broader aspects of his work and some of his studies have been carried out jointly with Professor P. W. Bridgman, as well as with other members of the physics department. He has

—Harris & Ewing

PRESIDENT HAROLD WILLIS DODDS OF PRINCETON UNIVERSITY

also received the cooperation of members of the staff of the department of physiology and of the medical school.

In 1929 Dr. Conant was elected a member of the National Academy of Sciences and the following year he was appointed a scientific director of the Rockefeller Institute.

Professor Harold Willis Dodds, who has assumed the presidency of Princeton University, is only three years older than Professor Conant. He is Princeton's fifteenth president and the youngest the university has had for 175 years. Born in Utica, Pennsylvania, he attended Grove City College, where his father, a Presbyterian minister, was Bible professor. Thirteen of the fourteen preceding presidents had been clergymen. The one previous exception was Woodrow Wilson, who also entered the chief office of the university by way of a professorship in politics. Wilson's father also was a clergyman.

Professor Dodds taught school for two years before beginning his graduate work at Princeton, where he took his master's degree in 1914. He became instructor in economics at Purdue University and later moved to Philadelphia, where the University of Pennsylvania granted him his doctor's degree in 1917. During the war-period he acted as executive secretary of the State of Pennsylvania for the U. S. Food Administration. He then served successively as member of the faculty of Western Reserve University (as assistant professor of politics), the University of Pennsylvania (as part-time lecturer), Swarthmore College (as part-time lecturer), New York University (as part-time lec-

CHANCELLOR HARRY WOODBURN CHASE OF NEW YORK UNIVERSITY

turer) and Princeton University, where he was professor of politics. He devoted most of his time to the National Municipal League from 1920 to 1928 when he served as executive secretary and editor of its journal, *The National Municipal Review*. He retained the latter position until this year.

In 1922, on the recommendation of Secretary Charles Evans Hughes, Professor Dodds was appointed as electoral adviser to the government of Nicaragua. His first duty was to formulate an electoral law for the country which, little altered, is still known to its citizens as the "Dodds Law." Two years later President Diaz called upon him to supervise registration for the presidential election in Nicaragua, which then enjoyed one of the most peaceful election days in its history. Professor Dodds rendered further service to Latin-American countries by serving as technical adviser in 1925 to the Tacna-Arica Plebiscitary Commission headed by General John J. Pershing.

Dr. Harry Woodburn Chase, former president of the University of Illinois and of the University of North Carolina, became the eighth chancellor of New York University on July 1, when he succeeded Dr. Elmer Ellsworth Brown.

During the past fourteen years, Chancellor Chase has become an outstanding university administrator. Under his leadership the University of North Carolina attained its position as one of the most progressive institutions of higher learning in the South. As president of the University of Illinois for the past

three years, he pushed forward plans for the promotion of the university's service to the state in the face of a decreasing budget.

Dr. Chase was born at Groveland, Massachusetts, fifty years ago. He was graduated from Dartmouth College and in 1908 received his master's degree from this institution. He obtained his Ph.D. from Clark University, where for a short time he was director of the clinic for sub-normal children. In 1910 he joined the faculty of the University of North Carolina as professor of the philosophy of education under the late Dr. Edward Kidder Graham.

Upon the death of Dr. Graham in 1918, Dr. Chase, then a professor of psychology, became in quick succession acting dean, chairman of the faculty and acting president of the university. While the Board of Regents was casting about to find a president to carry out the reorganization program that Dr. Graham had planned for the institution at Chapel Hill, Dr. Chase was so proving his success as an administrator that in 1920 he was selected by the Board of Regents as the best man for the post.

In 1930 Dr. Chase accepted the presidency of the University of Illinois, and it was this position he left to fill the chancellorship of New York University.

WILLIAM BEAUMONT

THE FIRST REPRODUCTION OF AN OLD DAGUERREOTYPE LOANED BY DR. HARRIS A. HOUGHTON, OF THE NEW YORK ACADEMY OF MEDICINE.

IN HONOR OF WILLIAM BEAUMONT

EARLY in October the New York Academy of Medicine was the scene of the celebration of the one hundredth anniversary of the publication of William Beaumont's "Experiments and Observations on the Gastric Juice and the Physiology of Digestion." In commenting upon the book, Professor Walter B. Cannon, of Harvard University, said:

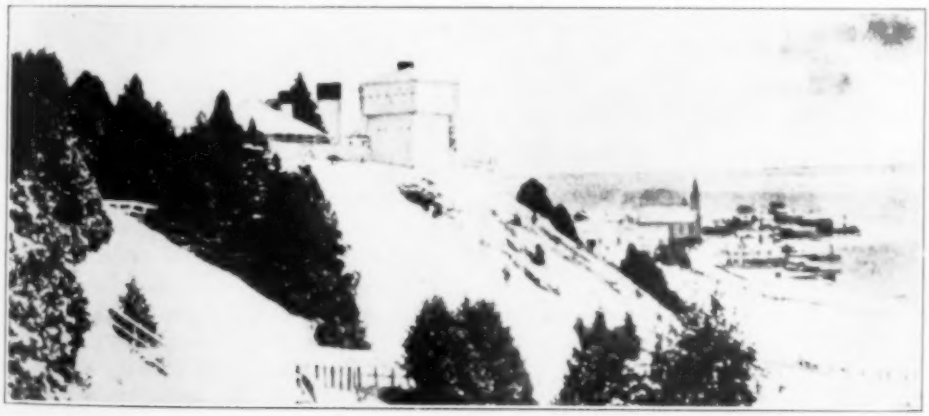
In the period between 1833 and 1933 thousands of other books had been written and published, had had their brief day and ceased to be. What was there in Beaumont's writing that endowed it with vitality and permanent value? It possessed those qualities because it

embodied the simple, straightforward report of a scrupulously honest man who used his senses cautiously in a significant scientific inquiry, who recorded exactly how he used them and what they revealed, and who drew limited inferences from the observed facts. He had, to be sure, an unusual condition to study and describe . . . a human being with a direct opening into the stomach on the left side of the body, through which instruments and food could be introduced and through which also the gastric contents and the digestive juice could be extracted. . . .

The conditions which surrounded him were in many respects highly unfavorable both to the prosecution of research and to the securing of satisfactory results. It was at a frontier army post on the Island of Machilimacinac, near the union of Lake Michigan and Lake Huron, that the young Canadian hunter, Alexis St. Martin, received the gun shot which fractured ribs and made openings into the cavities of the chest and abdomen. Portions of the lungs and stomach, much lacerated and burnt, protruded through the openings, making, according to the record, "an appalling and hopeless case." The life of the wounded man was despaired of, but, by careful attention and treatment, and, no doubt in part because of his own youthful vigor, he recovered, in about ten months, sufficiently to promise ultimate survival. Even at that time, however, he was "altogether miserable and helpless," and since no one else would look after his needs, Beaumont took him into his own home and "medically and surgically treated and sustained him, at much inconvenience and expense, for nearly two years, dressing his wounds daily, and for a considerable part of the time twice a day, nursed him, fed him, clothed him, lodged him and furnished him with such necessities and comforts as his conditions and sufferings required."

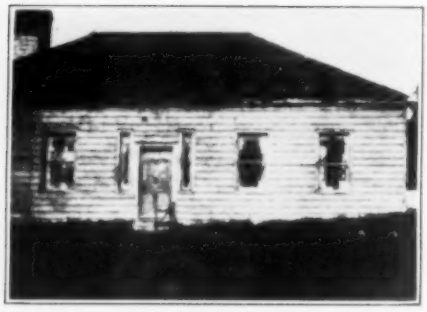
The unique contract between William Beaumont and St. Martin was described by Dr. Harris A. Houghton, chairman of the celebration. The second part of it is quoted here.

And the said Alexis, for the consideration hereinafter mentioned, further especially covenants and agrees with said William, that he, the said Alexis, will at all times during said term, when thereto directed or required by said William, submit to, assist and promote by all means in his power, such Physiological or Medical experiments, as the said William shall direct or cause to be made on or in the stomach of him, the said Alexis, either through or by the means of, the aperture or opening thereto in the side of him, the said Alexis, or otherwise, and will obey, suffer and comply with all reasonable and proper orders or experiments of the said William, in relation thereto, and in relation to the exhibiting and showing of his said stomach, and the powers and properties thereof, and of the appurtenances and powers, properties, situation and state of the contents thereof. It being intended and understood both by said William and said Alexis that the facilities and means afforded by the wounds of the said Alexis in his side and stomach shall be reasonably and properly used and exhibited at all times upon the request or direction of said William for the purpose of science and scientific improvements,



FORT MICHILIMACKINAC IN MIDWINTER

SHOWING ONE OF THE BLOCK-HOUSES, WITH THE SURGEON'S QUARTERS AND HOSPITAL TO THE LEFT OF IT.



THE ONE-STORY FRAME HOSPITAL AT THE OLD FORT

TO WHICH ALEXIS WAS REMOVED, AND WHERE HE LAY DURING HIS LONG CONVALESCENCE.

the furtherance of knowledge in regard to the power, properties and capacity of the human Stomach.

Dr. Bernard Sachs, president of the academy, opened the meeting with a brief address, in which he related the salient points in Beaumont's unusual career. William Beaumont as an army officer was the subject of an address by Major General Robert U. Patterson.

An exhibition of books and other memorabilia relating to the life, work and travels of Beaumont will be on view at the New York Academy of Medicine until November 3.

AN ELECTRICAL SURVEY OF ISLANDS AND CONTINENTS IN INVISIBLE FILMS ON THE SURFACE OF WATER

SURFACES, or more properly interfaces, are almost all important in a study of the action of the muscles, nerves, blood and all the other parts or materials of the human body. In a research begun several years ago as supported by the Julius Stieglitz Fund of the Chemical Foundation, William D. Harkins and E. K. Fischer have begun a study of the fundamental characteristics of membranes, especially of the electrical relations. As a part of this work the earlier methods have been modified in such a way as to enable them to give fundamental information concerning the forces which hold molecules together, the orientation of molecules in thin films, and the structure of the different regions of a film.

The surfaces of all natural and artificial bodies of water are covered, more or less, by exceedingly thin invisible or sub-ultra-microscopic films of oily substances. These invisible films are very much thinner than those often seen, which are visible layers of oil, highly colored by interference of the light waves which traverse them, and are about 1/25,000 of an inch in thickness. It has been known for many years that electrical methods may be utilized in the study of such films, but the results obtained were not only disappointing but conflicting. About five years ago Professor Harkins suggested that the discord in the results might disappear if both electrical measurements and measurements of the lateral pressure of the film were made simultaneously.

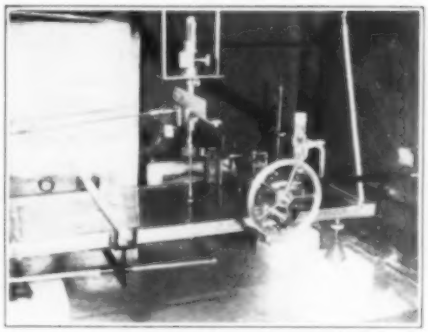
It is a remarkable fact that an oil film only one molecule thick, that is, with a thickness of only about one tenth of a millionth of an inch, is capable of transmitting a lateral pressure which is large enough to move a floating sheet of metal, and to do this over a considerable number of inches. Such a film may exhibit many of the characteristics of a solid, and is then called a monomolecular or unimolecular solid film, or it may act in the two dimensions of the surface as a liquid or as a gaseous film. The Hardy-Harkins-Langmuir theory of surfaces indicates that with films produced from oils, such as oleic or stearic acid, the molecules are oriented, as are the soldiers in a troop on parade. This orientation is most manifest when the molecules are tightly pressed together to form a condensed

film, since in this case very few molecules get a chance to "lie down on the surface." In gaseous films this orientation is partly but not wholly preserved.

In the oil film the water-like heads of the molecules turn downward toward the surface of the water, while the oil-like tails of the molecules project upward. The electrical tests indicate that the heads of the molecules are negatively charged with respect to the more positive tails.

One of the three cells of the storage battery used in an automobile gives a potential of about two volts. It is somewhat remarkable that the presence of a film of oil one molecule (0.0000001 inch) thick may change the potential difference at the surface by as much as about one half a volt.

The greatest practical use of the apparatus used in the electrical testing of a surface is to make a topographic survey of a water surface. The surface potential is found to rise rapidly as the electrode, held a few millimeters above the surface, approaches the edge of an invisible island or a continent of closely packed molecules of the oil; the potential remains high while the electrode is over the island, and falls again as the ocean of gaseous film is approached. Thus, what is invisible to the eye and to the microscope is revealed by the movement of the needle of the electrometer which is used to detect the change of potential. This motion is in turn shown by the movement of a beam of light.



APPARATUS FOR FINDING ISLANDS OF INVISIBLE FILM ON WATER

Certain large molecules, about 60 times larger than the rather large molecules usually used to produce films, have exhibited the relation that they form films of only one fourth the thickness of those produced by the smaller molecules. Therefore, these large molecules, which give a film only one fiftieth of a millionth of an inch in thickness, must lie flat on the surface.

RECENT DEVELOPMENTS IN THE STUDY OF CHEMICAL EFFECTS OF ALPHA PARTICLES AND ELECTRONS

WHEN alpha rays from radon or other radioactive elements pass through the molecules of substances, they ionize them by removing electrons temporarily. Chemical action frequently results and has been found to be dependent on and proportional to the number of ions formed, though the proportionality factor is not the same as in Faraday's laws of electrolysis.

Similarly, if gas ions are produced in gases by electrical discharge, chemical action similar to that in radon is produced, in the bosom of the gas, however, not at the electrode. In different substances under parallel conditions of discharge the amount of reaction bears the same relations as in the same reactions under alpha radiation. This leads to the assumption that the number of molecules reacting per ion is the same in both cases, though one is not able to measure nor calculate the ionization in electrical discharge.

Such studies have now been extended to perhaps a hundred different reactions under alpha radiation and to many more in electrical discharge. Recent investi-

gations carried out either in the laboratory of Professor Mund, at the University of Louvain in Belgium, or in the School of Chemistry under the directorship of Professor S. C. Lind, at the University of Minnesota, which are the two centers where radium is being devoted to this purpose, include the following reactions: The synthesis and decomposition of hydrogen iodide, hydrogen bromide, hydrogen chloride, hydrogen sulfide and of ammonia, the synthesis of water at high and low temperatures (-180 to $+500°$ C.) and the polymerization of gaseous derivatives of acetylene.

Much of recent research is directed toward the mechanism of the reactions, whether the gas ions are themselves the activated agents that react directly or whether they first give free radicals and free atoms that enter into reactions. There is much indirect evidence for both views with no possibility of a convincing decision in sight.

The influence of foreign non-reactive gases is important. They are also ionized by the alpha particles and their ions contribute as much to the reaction as ions of the reactant, even when the foreign gas consists of single inert atoms like the inert gases. This indicates strongly that the reaction is a clustering process about the positive ion of the inert gas or that by an exchange of ionization on collision the inert gas ions ionize and activate the reactant. This is impossible, however, when the ionization potential of the inert gas, like xenon, is lower than that of the reactant gas, like acetylene. And yet, xenon ions do cause acetylene molecules to polymerize with high efficiency. This argument is almost, but not quite, conclusive that the ions themselves are the reactive agents.

All foreign ions are not equally efficient in promoting a given reaction. While many of them are 100 per cent. efficient, carbon dioxide is only 14 per cent. efficient in its own synthesis from carbon monoxide and oxygen, while water vapor is 100 per cent. efficient in its synthesis from hydrogen and oxygen —two reactions which under alpha radiation are entirely similar, except in this one respect, even having identical yields per ion.

The factor on which the efficiency of foreign ions depends is not definitely known, but Mund has presented recent evidence that it may depend on the dielectric constant of the gas concerned.